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THE ORIGIN OF BUTTERCUPS.

By Grant Allen.

HERE, in my hand, I hold a solitary little golden buttercup, picked this morning in an autumn meadow, but still as bright and sturdy as though it had grown up in warmer days beneath the sunny skies of June. Common and familiar as it is, the buttercup is yet a very interesting flower from the point of view of its origin and evolution. Not that it is a highly-evolved or very singular blossom, with a long and intricate history at its back, like some of the orchids and snapdragons, whose complexity almost defies explanation; on the contrary, the importance of the buttercup in the eyes of the historical botanist is mainly due to the extreme simplicity of its typical arrangement. It is a very early type of plant, which has scarcely undergone any alteration from the form it must have acquired already many millions of years ago. There are other flowers of the same family, such as the larkspur, the columbine, and the monkshood, which still bear obvious traces of being derived from an ancestor exactly like the buttercup, but which have diverged widely from the original stock in their curious irregular flowers, sometimes spurred, sometimes hooded, and sometimes so altered from the primitive radial shape as to be scarcely recognisable. What makes our buttercup so interesting, on the other hand, is the fact that it represents an early stage in the history of these more highly-developed forms. In order to understand *them* we must first understand *it*. This buttercup, in short, is one of the most central members of the family to which it belongs; while some of its congeners have diverged in one direction and some in another, it has still kept unaltered for us the primitive lineaments of the common ancestor from which all alike have ultimately sprung.

Buttercups, as everybody knows, are tall meadow weeds, and the one which I hold in my hand belongs to the tallest species of all, which we know *par excellence* as *the* buttercup; for we have in England alone no less than some sixteen representatives of the entire genus. Let us look a little closely into its structure, and see what hints we can gather from its existing shape as to its past history and evolution.

First of all there are the leaves. These, one notices at once, are raised on long stalks, and deeply divided into several segments. Sometimes there are only three divisions to each leaf, sometimes five, and sometimes seven; the reason why they thus run in uneven numbers being, of course, that there is always a single terminal leaflet together, with one, two, or three lateral leaflets or either side of it. Again, each of these segments is itself further divided into three toothed lobes. Now, such a complex leaf as this shows by its very nature that it must be the product of considerable previous development. All very early leaves are quite simple and rounded; it is only by slow steps that a leaf thus gets broken up into many divided segments. In this respect, then, the meadow buttercup cannot be regarded as the simplest member of its class. There are some other buttercups, such as the ivy-leaved crowfoot, which creeps along the mud of ditches, or the lesser celandine, which springs in the meadows in early April, whose leaves are entire and undivided. In the lesser celandine they are almost circular, and in the ivy-leaved crowfoot they are slightly angular; but both these plants, having plenty of room to spread in the unoccupied fields of spring or the unappropriated ditches, have never felt the necessity for subdivision into minute segments. They have free access to the air and the sunlight, and so they can assimilate to their hearts' content the carbon of which their tissues are built up. It is otherwise, however, when similar plants push out into new situations, less fully supplied with carbonic acid or with sunshine. For example, there is the water-crowfoot, a mere divergent variety of the ivy-leaved species, which has taken to growing in ponds or rivers. Here it cannot obtain the materials for growth so readily as on its native mud-banks; and it has been compelled, accordingly, to split up its submerged leaves into long, thin, hair-like filaments; but when it reaches the surface, its foliage spreads out once more into the broad ancestral blades of the ivy-leaved crowfoot. It is just the same with the true buttercups. They have taken to growing in the open meadows, where the competition for vegetable food-stuffs is keen, and the struggle for existence very bitter. Hence they have been compelled to divide their leaves into many finger-like segments; and only those which have succeeded in doing so have managed to hold their own in the struggle, and so to hand down their peculiarities to future generations. As a rule, just in proportion as vegetation is thick and matted, do the plants of which it is composed tend to develope minutely divided and attenuated foliage.

It is the flower, however, that most people think of as the essential part of a buttercup, and it is by means of the flower that all the higher plants are usually classified. Now, the blossom of the buttercup is almost an ideally simple typical specimen. It consists of three parts or series of organs, from within outward. First comes a little central boss or cushion, supporting several carpels or unripe fruitlets. Each of these carpels contains a single embryo seed. Outside these comes a row of many stamens, which are the organs for producing the yellow dust which we call pollen. Now, no carpel can mature into a fruit containing ripe seed until it has been impregnated by pollen from a stamen, and these two sets of organs are, therefore, the only really essential parts of the whole flower. But in common language, what we mean by a flower is not these little central knobs and tassels, but rather the bright-coloured petals outside, which in the buttercup are five in number and golden yellow in colour. What, then, is the use to the plant of these expanded and very strikingly-coloured organs?

A flower is at bottom merely a device for producing seed. But in order that the seed may prove capable of germinat-

ing, the ovules in its carpels must necessarily be fertilised by pollen. Now, all the earliest flowers consisted merely of stamens and carpels; they had no petals at all. But, as Mr. Darwin has shown, flowers which are fertilised by pollen from a neighbouring plant produce more seed and healthier seedlings than those which are fertilised by the produce of their own stamens. Hence, any modification which promoted such cross-fertilisation would benefit the plants in which it occurred by giving them an advantage over their rivals in the struggle for existence. Now, there are two ways in which flowers have thus acquired special adaptations for fertilising one another. Some of them have developed hanging stamens which shake out their pollen to the wind, and such flowers are also provided with feathery collecting surfaces to the carpels, so as to catch the stray grains which may happen to be wafted to them from their neighbours by the breeze. Plants of this type never possess bright-coloured petals. A second class, on the other hand, have learned to utilise the winged insects which visit their blossoms in search of food. These welcome little pilferers, in passing from head to head, carry the pollen of one plant to the carpels of another, and so assist the flower in effecting the desired cross-fertilisation. This class, to which the buttercup belongs, has usually developed various inducements of food, scent, and colour, in order to attract the fertilising insects. Those flowers which best succeeded in alluring their little winged guests would naturally hold their own against all less highly endowed competitors, and would hand on their own constitution to their descendants. In this way insect-fertilised plants have acquired the bright petals and sweet scents which chiefly make them noticeable to our human senses. A brief examination of the buttercup blossom will show us the use which they subserve, and the way in which they act in the simplest forms of insect-fertilised flowers.

Pull out one of the golden petals from the outside of the head, and you will see at its base a small hollow spot, covered by a tiny concave scale. That spot is the nectary, and it contains a single drop of pure honey. The honey is put there to entice bees and other insects; it is the wage offered them by the plant in return for the service which they perform for it in fertilising its seeds. The golden hue of the petals, again, acts as an advertisement for the honey; the bees know that such bright hues are never found in any flowers except those which specially lay themselves out to bid for their favour. When a bee sees the brilliant colour, he flies straight towards the blossom and settles on the little boss of carpels in the centre. Here he sips the honey for his own behoof, and at the same time dusts himself with pollen on behalf of the flower; for, as soon as the blossom opens, the stamens discharge their precious burden, beginning from the outside and slowly ripening towards the centre. At this early stage, however, the carpels are not yet mature for impregnation, and so they avoid being fertilised from the pollen of their own stamens. If the bee flies away to another buttercup which happens to be still in the same stage of development, he only collects more pollen about his head and thighs; but if he alights on a somewhat older buttercup, he finds its stamens withered and its carpels fully mature for impregnation. Some of the pollen is then sure to fall on the sensitive surface of the carpels. Thus, while he seeks honey for himself, he unconsciously affords his host all the advantages of cross-fertilisation; and it is because he does so that the flower has been enabled to develope its complicated arrangement of petals and nectaries for his delectation.

The buttercup, then, with its five separate simple petals, its many stamens, and its central one-seeded carpels, may be regarded as a good example of the earliest type of insect-fertilised flowers. In some other plants, such as the harebell and the primrose, the separate petals have coalesced into a single tubular corolla; while in others, again, they have assumed various fantastic shapes; but all of them are ultimately derived from flowers like the buttercup, which thus contains in itself all the essential elements of a perfect insect-fertilised plant.

SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART II.

MULTITUDES of examples may be cited illustrating the viscosity of bodies that we usually regard as types of solidity, such, for example, as the rocks forming the earth's crust. In the "Black Country" of South Staffordshire, which is undermined by the great ten-yard coal-seam, cottages, chimney-shafts, and other buildings may be seen leaning over most grotesquely, houses split down the middle by the subsidence or inclination of one side, great hollows in fields or across roads that were once flat, and a variety of other distortions, due to the sinking of the gradual rock-strata that have been undermined by the colliery workings. In some cases the rocks are split, but usually the subsidence is a gradual bending or flowing down of the rocks to fill up the vacuity, as water fills a hollow or "finds its own level."

I have seen many cases of the downward curvature of the roof of a coal-pit, and have been told that in some cases the surrounding pressure causes the floor to curve upwards, but have not seen this.

As Knowledge will doubtless have many readers in the colliery districts, some of them may be able to supply reliable evidence of this, accompanied with careful measurement of the amount of upward curvature or upheaving in a stated breadth of road or working.

Earthquakes afford another example. The so-called solid crust of the earth is upheaved and cast into positive billows that wave away on all sides from the centre of disturbance. The earth-billows of the great Lisbon earthquake of 1755 travelled to this country, and when they reached Loch Lomond, were still of sufficient magnitude to raise and lower its banks through a perpendicular range of two feet four inches.

It is quite possible, or, I may say, probable, that there are tides of the earth as well as of the waters, and the subject has occupied much attention and raised some discussion among mathematicians. If the earth has a fluid centre, and only a comparatively thin crust, as some suppose, there must be such tides, produced by the gravitation of the moon and the sun.

Ice presents some interesting results of this viscosity. At a certain height, varying with latitude, aspect, &c., we reach "the snow line" of mountain slopes, above which the snow of winter remains unmelted during summer, and, in most cases, goes on accumulating. It soon loses its flocculent, flakey character, and becomes coherent, clear blue ice by the pressure of its own weight.

A rather complex theory has been propounded to explain this change—the theory of *regelation*—*i.e.*, refreezing, a theory which assumes that the pressure between the surfaces first thaws a film of ice at the surfaces of contact, and that presently this refreezes, and thus effects a heating or general solidification. Faraday found that two pieces of ice with moistened surfaces united if pressed together

when at just about the temperature of freezing, but not if much colder. Tyndall has further illustrated this by taking fragments of ice and squeezing them in a mould, whereby they became a clear, transparent ball, or cake, and schoolboys did the like long before when snowballing with snow at about the thawing point. Such snow, as we all remember, became converted into stony lumps when firmly pressed together. We also remember that in much colder weather no such cohesion occurred, but our snowballs remained powdery in spite of all our squeezing.

I am a sceptic as regards regelation. I believe that the true explanation is much simpler; that the crystals of snow or fragments of ice in these experiments are simply welded, as the smith unites two pieces of iron, by merely pressing them together when they are near their melting point. Other metals and other fusible substances may be similarly welded, provided they soften or become sufficiently viscous before fusing. Platinum is a good example of this. It is infusible in ordinary furnaces, but becomes pasty before melting, and, therefore, one method adopted in the manufacture of platinum ingots or bars from the ore, is to precipitate a sort of platinum snow (spongy platinum) from its solution in acid, and then compress this metallic snow in red-hot steel moulds by means of pistons driven with great force. The flocculent metal thus becomes a solid, coherent mass, just as the flocculent ice became coherent ice in Tyndall's experiment or in making hard snowballs. Wax, pitch, resin, and all other solids that fuse, *gradually* cohere, are weldable, or, in very plain language, "stick together" when near their fusing point.

I have made the following experiment to prove that when this so-called regelation of snow or ice fragments occurs, the ice is viscous or plastic, like wax or pitch. A strong iron squirt, with a cylindrical bore of half-an-inch in diameter, is fitted with an iron piston. This piston is driven forth by a screw working in a collar at one end of the squirt. Into the other end is screwed a brass nozzle with an aperture about one-twentieth of an inch diameter, tapering or opening inwards gradually to the half-inch bore.

Into this bore I place snow or fragments of ice, then holding the body of the squirt firmly in a vice, I work the lever of the screw, and thus drive forward the piston and crush down the snow or ice fragments, which presently become coherent and form a half-inch solid cylinder of clear ice. Applying still more pressure, this cylinder is forced like a liquid through the small orifice of the nozzle of the squirt, and jets or sprouts out as a thin stick of ice like vermicelli, or the "leads" of ever-pointed pencils, for the moulding of which the squirt was originally constructed.

I find that ice at 32° can thus be squirted more easily than bees'-wax of the same temperature, and such being the case, I see no reason for imagining any complex operation of regelation in the case of the ice, but merely regard the adhesion of two pieces of ice when pressed together as similar to the sticking together of two pieces of cobblers'-wax, or softened sealing-wax, or bees'-wax, or the welding of iron or glass when heated to their welding temperatures, *i.e.*, to a certain degree of incipient fluidity or viscosity.

If a leaden bullet be cut in half, and the two fresh-cut faces pressed forcibly together, they cohere at ordinary atmospheric temperatures, but we have no occasion for regelation here. The viscosity of the lead accounts for all. At Woolwich Arsenal there is a monster squirt, similar to my little one. This is charged with lead, and, by means of hydraulic pressure, the lead is squirted out of the nozzle as a cylindrical jet of any required diameter. This jet or stick of lead is the material of which the elongated cylindrical rifle bullets are now made.

But returning to the point at which we started, on the subject of ice, viz., its Alpine accumulation above the snow-line. If the snow-fall there exceed the amount that is thawed and evaporated, it must either go on growing upward until it reaches the highest cloud, or atmospheric region, from which it falls, or is formed, or it must descend somehow.

If ice can be squirted through a syringe by mere hand-pressure, we are justified in expecting that it would be forced down a hill slope, or through a gully, or across a plain, by the pressure of its own weight when the accumulation is great. Such is the case, and thus are glaciers formed.

They are, strictly speaking, rivers or torrents of ice; they flow as liquid water does, and down the same channels as would carry the liquid surface drainage of the hills, were rain to take the place of snow. Like rivers, they flow with varying speed, according to the slope; like rivers, their current is more rapid in the middle than the sides; like rivers, they exert their greatest tearing force when squeezed through narrow gullies; and, like rivers, they spread out into lakes when they come upon an open basin-like valley, with narrow outlet.

The *Justedalsbrae* of Norway is a great ice-lake of this character, covering a surface of about 500 square miles, and pouring down its ice-torrents on every side, wherever there is a notch or valley descending from the table-land it covers. The rate of flow of such downpouring glaciers varies from two or three inches to as many feet per day, and they present magnificent examples of the actual fluidity or viscosity of an apparently solid mass. This viscosity has been disputed, and attempts have been made to otherwise explain the motion of glaciers, but while it is possible that it may be assisted by varying expansion and contraction, the downflow due to viscosity is now recognised as unquestionably the main factor of glacier motion.

Cascades of ice may be sometimes seen. In the course of my first visit to Norway, I wandered alone over a very desolate mountain region towards the head of the Justedal, and unexpectedly came upon a gloomy lake, the Styggevand, which lies at the foot of a precipice-boundary of the great ice-field above-named. Here, the ice having no sloping valley-trough by which to descend, poured over the edge of the precipice as a great overhanging sheet or cornice, which bent down as it was pushed forward, and presented on the convex side of the sheet some fine blue cracks, or "crevasses" as they are called. These gradually widened and deepened, until the overhanging mass broke off and fell into the lake, on the surface of which I saw the result, in the form of several floating icebergs that had previously fallen.

Something like this on a small scale may be seen at home on the edge of a house roof, on which there has been an accumulation of snow; but, in this case, it is rather sliding than flowing that has made the cornice; but its *down-bending* is a result of viscosity.

GERMS OF DISEASE AND DEATH.

By Dr. Andrew Wilson, F.R.S.E.

MOST readers have heard of the "Germ theory," and there are few persons who do not know what the hypothesis of that name means and implies. Popularly regarded, this theory holds that a very large proportion of the diseases that affect and afflict man and his neighbour animals, owe their origin to minute forms of life—whether animal or vegetable, or both, is still, in most cases, a

matter of doubt. To select a single illustration of the application of this theory, we may take the case of small-pox, and its analogous condition, the fever produced by vaccination. When an infant is vaccinated, the physician introduces into its system, through an abrasion of its skin, a minute quantity of vaccine lymph, which, as everybody knows, is obtained either from the vaccination pustule of an already vaccinated child, or direct from the calf. In either case, there are introduced into the infant body, certain minute germs—suspended in and living naturally amongst the vaccine lymph—and in due course these germs multiply and increase within the frame, thereby producing the characteristic fever, and the equally characteristic pustule at the seat of the operation. So, also, with small-pox, which vaccination imitates in a mild way, and of which, moreover, it is a preventive. Here the germs of small-pox, obtained directly or indirectly from an already infected person, attack the body. Gaining admittance thereto, they propagate themselves within the tissues and through the medium of the blood. Sooner or later all the characteristic symptoms of the disease are manifested, and having run its course, it dies away as mysteriously, to all appearance, as it came. Now, there is something strikingly analogous in all this to the growth of an animal or plant. There is a period of "incubation" in the fever, just as in the production of the living being there is a period of development. There is a growth of the fever, as the animal or plant grows towards its maturity; and there is a decline of the disease, as the living form passes to its old age and death. So far, then, the parallel between ordinary life and the birth, growth, and decline of a disease, is very close and clear.

But the analogies are not yet exhausted. Each fever produces its like, as do animals and plants. Each disease reproduces its kind, as Tyndall has somewhere observed,* as rigorously as dog and cat reproduce their like. The phenomena, or, as a doctor would call them, the "symptoms," of each disease are, as a rule, highly distinctive. The symptoms of scarlatina are not those of small-pox; measles is different from the other two; whilst typhus fever is again thoroughly different from all three. Analogy may, as Darwin says, be a deceitful guide; but when the facts are so closely allied, as are the facts of epidemic diseases to those of animal and plant development, the use of analogy cannot be doubted in rendering the relationship clearer.

We are now in a position to understand more clearly the utility and strength of the germ theory in certain of those aspects which bear most materially on science at large. It would only serve to strengthen the idea that our epidemic diseases are simply the offspring of lower life, if we reflect in passing that there are known to science a very considerable number of lower plants which produce in man's skin effects and diseases as characteristic as those which a fever induces in his system at large. Thus, the disease known as "ringworm" is caused by the growth in the human skin of a parasitic fungus, and a whole series of skin affections is known wherein lower plants play the part of direct causes. Thus, if it is a matter of certainty that a particular skin-disease is caused by lower plant-growth, so no less is it by analogy likely that all other contagious and epidemic diseases are in reality the products of life.

So much for the general idea that permeates the "germ-theory" of disease. Within the past few months some highly important additions have been made to our knowledge of the part played by lower organisms in the production of disease. M. Pasteur, whose researches into the development of lower organisms have placed him in the foremost rank of scientific workers, has detailed at length the results of his investigations into the causes which produce the curious disease known as *charbon*, *anthrax*, and *splenic fever*. This disease, whilst but rarely attacking man, is fatal to horses, cattle, and sheep. France suffers greatly from this "plague of boils," and it is also known in various other countries as a literal scourge. Pasteur, it should be mentioned, had already acquired much valuable experience in the investigation into the cause of the *pébrine*, or silkworm disease, which in 1863 had devastated the silk industry of France. Pasteur showed that *pébrine* was caused by the growth and multiplication, within the bodies of the insects, of minute "corpuscles," which were practically lower forms of life. Even the eggs from which the worms were hatched were shown to be liable to infection from *pébrine;* the eggs, in such a case, inheriting the disease from the parent moth which laid them. As the result of a long and laborious series of experiments, Pasteur showed that the *pébrine* would spread like an infectious disease by the contact of whole with diseased worms. He showed that, just as man isolates his fever patients, so the French silk-grower had to isolate and separate his diseased worms. But the knowledge which led to this effectual result was knowledge that had been won by an uphill fight, and that had been gained by the object-glass of the microscope, and by the whole-souled devotion of many months' industry.

* Quoting a remark by Miss Nightingale.—Ed.

THE LAWS OF PROBABILITY.

THERE are few subjects in which men take a more general interest, yet few in which they make greater mistakes, than the subject of probabilities. From the man whose mind is most perfectly trained in the analysis of evidence, down to him whose thinking apparatus can scarcely be regarded as a mind at all, all men endeavour to guide their conduct in matters uncertain, or at least to form their opinion on such matters, according to the probabilities. They would use different words in describing their purpose. A mathematician might, perhaps, speak definitely of the *à priori* and *à posteriori* probabilities in favour of an opinion or of a course of conduct. Wendell Holmes' "lout," he "who lies outstretched on a tavern bench with just mental activity enough to keep his pipe from going out," would hardly use the same expressions; but in his imperfect way he is all his life doing the same thing. He does not even stretch out his ungainly limbs on one bench rather than on another, or in one attitude rather than another, without to some degree considering his chance of comfort or security. But in all the ordinary relations of life he more definitely weighs the chances, though often, if not always, in an utterly inexact balance.

It is not the loutish mind only, however, nor the average intellect, which inexactly estimates probabilities. The most profound knowledge of the mathematics of chance does not save men from error. Often, indeed, the grossest blunders have resulted from an attempt to weigh ordinary matters in a mathematical balance. The fault has not lain, however, with the balance, but with the user. If a man chooses to weigh groceries in a chemical balance, paying no attention to the fact that either the parcels he weighs come in contact with other things lying round, so that the balance cannot possibly show their true weight, or else, perhaps, that the construction of the balance is such as will only permit of its indicating true results within certain very

narrow limits of weight, he will obtain a much less satisfactory result than he would if he used a common weighing machine incapable of showing differences of a few grains or scruples. Yet he should not condemn the balance, but himself for misusing it.

Not unfrequently mathematicians have fallen into a parallel error. Indeed, there are those who assert that mathematicians are more apt than other men to err in this way, because they try to apply the exact laws of probability to matters about which they only have inexact knowledge. To use an illustration of Huxley's, mathematicians possess a mill of exquisite perfection, which grinds you stuff of any degree of fineness, but they are apt to put peacods into it and to regard the product of their grinding as wheaten flour.

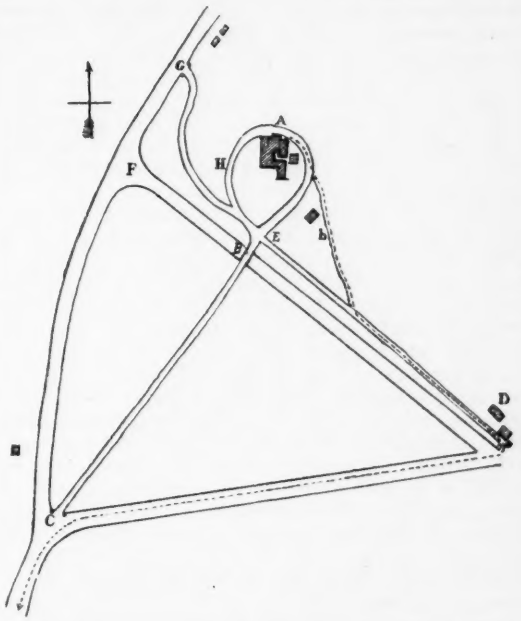
However, there is no real connection between profound mathematical research and utter absence of common sense. Mathematicians do not often err in this way. The laws of probability would be worth knowing even if they did. Nor is the application of these laws to many cases where they really are applicable likely to detract from what is commonly called the common sense way of viewing matters. Indeed, the common sense method may be regarded as only a rough way of applying the just method. The probability of its leading to a tolerably correct result is much less than the probability of a correct result when due account is taken of details; just as the chance of correctly estimating the cost of a complicated work by roughly adding a number of roughly-estimated items is less than the chance of a correct estimate when these items, separately estimated as exactly as possible, are properly added together. In neither case do we get an exact result, but the chances of gross error are much greater in the former than in the latter; for to the chances of error arising from an exact estimate of the several items in the latter case there is added the chance of a large error in a rough estimate of the totality of these items.

It must be admitted that by justly combining the various probabilities on which a question depends, we are more likely to attain a correct opinion than if we formed only a rough idea of what would result from such a combination. Yet the incautious application of the laws of probability, combined with undue reliance on results which, at the most, had been only shown to be probably true, has often led to absurd blunders.

INTELLIGENCE IN ANIMALS.

PASSING over several cases which seem to carry the matter no further than those already cited, we come next to a case which appears to us one of the most striking ever recorded. The writer, Mr. E. H. Pringle, remarks that it is an instance of sagacity which finally set at rest any doubts he had ever entertained that the difference between human and animal intelligence is one of degree only. We can see no way in which the story can be explained without assuming the exercise of something more than that mere practical reasoning which probably underlies all the so-called instinctive actions of animals :—

Mr. J. W. Cherry, of the Madras Forest Service, is owner of a dog, a bull-terrier, called "Bully." (This breed is notoriously not the most intelligent of the canine tribe, so that the behaviour of Bully appears all the more striking.) "We lived," says Mr. Pringle, "in the bungalow A,* the compound of which was bounded south and west by public roads DC and GFC, both leading to the cantonment of Bangalore in the direction C. There were three gates into the compound at C, D, and G, the main approach to the bungalow leading over a bridge B that spanned a public road FD. The compound was filled with trees and shrubs, and bordered by dense lantana hedges, so that with the exception of a portion of the western road at F, neither of the cantonment roads were visible from the bridge, nor could the footpaths be seen thence. Now, Bully had a lady friend (canine) living in the cantonment, and at times she was so attractive, that absences without leave on the part of the dog were frequent. After one of these excursions, Bully had been brought back and chained up for the night. Next morning, while his master and I were sitting at early breakfast, it was decided that he should be released, and to effectually



stop further delinquency, a peon was sent down to the bridge with orders to intercept him if he started for the cantonment. Bully was brought in and unchained; he had that unmistakable air of detected guilt deservedly punished, and spent some time in begging for scraps from the table in a most deprecatory manner. Shortly, however, he strolled into the verandah, and then down the front steps on to the gravel walk. After wandering about aimlessly for a few minutes, he quietly started down the approach AHB. We followed, keeping out of his sight. At the turn of the road, Bully met with the unexpected apparition of the peon standing on the bridge. In a moment, though not a word was spoken by the man, the dog turned and came straight back to the room, whither we had in the meantime slipped back unobserved, and re-entered it, wagging his tail violently, and looking exceedingly sheepish. He now lay down, and closed his eyes. The cocked ears showed that sleep was mere pretence, and he soon rose again, went out into the front garden, and hunted for buried bones—purely imaginary ones, I believe. His search gradually led him down the hill by a footpath leading from A to G, we keeping him in sight, as before, and he finally reached the road at the bottom. There all disguise was dropped, and he started off for the cantonment. As he neared the spot F, the peon

* The figure presents all the features essential to a correct understanding of Mr. Pringle's narrative.

espied him, and shouted out his name. He turned at once, climbed the hill, and came into the bungalow, where the same farce of repentance was gone through. Bully now seemed to have made up his mind that escape was impossible; he lay down on a mat in the verandah, and remained there for a long time. But for the persistent cock of the ears, we should have imagined the animal really asleep. Mr. Cherry eventually went to his office-room, and I remained in the verandah, reading the morning paper and occasionally glancing at Bully. He lay very still, but once or twice I detected him opening his eyes and raising his head to look round him. Each time he caught my eye he wagged his tail vehemently for a moment or two, and then resorted to his sham sleep. It may have been for half-an-hour or thereabouts that this state of things continued. I then became interested in an article in the paper, and when I next looked up, Bully was gone. I called Mr. Cherry, and the house was searched for Bully. The peon was sent for and interrogated; he had not seen the dog. As a last resource, inquiry was made of the horse-keepers down at the stables, D. The reply was "Yes, the dog had passed through the gate, D, some time before." Taking advantage of my occupation, and the absence of his master, Bully had left the house and taken his way to the cantonment by the only path by which he could have escaped unnoticed by the peon—that shown by the dotted line.

It seems to us quite impossible to account for the dog's action, as above narrated, without attributing to him the exercise of reasoning powers, not merely in selecting the route by which he finally escaped, but in the manœuvres by which he endeavoured to assure those who were watching him that he had given up all hope of escaping. Doubtless, if he had reasoned more perfectly, he would even have allowed his ears to seem asleep, instead of leaving them cocked. But very few of us human beings simulate sleep without some such error, by which any observant person would be enabled to detect the trick. Either the muscles of the face are not perfectly relaxed; or the hands or feet are left in a constrained attitude; or the position of the body generally is unlike that which a sleeping person would assume; or else the breathing is unnaturally restrained. And again, though Bully was too demonstrative in his contrition, and afterwards in his attempts to hide the consciousness of failure, it was not for want of reasoning power. Few of us know how to act such parts as he tried to play, with perfect correctness; nor do those who know succeed always in acting such parts as they could wish. Probably Bully was as sensible as the onlookers that he was not quite successful in his acting. It is obvious, however, that he directed his efforts as carefully to the end he wished to obtain as a human being of average reasoning powers and skill in counterfeiting sleep, &c., could have done.

ILLUSIONS.

By Thomas Foster.

IN my last paper there were two mistakes, or rather a single mistake, for it amounted really to the inversion of one of the figures. In Fig. 5, the lines marked AB, CD, should have been put opposite the letters EF, GH, and *vice versâ*. As they actually stand, the description should run as follows:—The lines AB, CD appear to be curved, so as to be farther apart at the middle than at either end; whilst the lines EF and GH, in the same figure, appear to be so curved as to be nearer in the middle than at either end.

Such illusions as are illustrated at pp. 11 and 12 vary in effect according to the position from which the illusory drawing is seen. Thus, if we so place Fig. 1 that the eye looks along, or nearly along, the direction AB, we see at once that CD is in the same straight line with AB. Again, although we cannot look simultaneously in the directions BC and ED in Fig. 3, it will be found that when we look along BC, so that the illusion ceases so far as the part of the circle near C is concerned, the illusion ceases altogether, the part near D no longer appearing to form part of a smaller circle. In Fig. 4, the illusion is not at all effective when we hold the lines AB, CD, &c., upright, but is very marked when they are looked at aslant. But again, even when we look at them aslant, and the illusion is strong, we yet find that running the eye backwards and forwards along the parallels, the illusion varies all the time; for the eye cannot fail then to perceive that the distance between the parallels does not vary as the illusory effect seems to suggest. Thus, the lines CD, EF, which, when the eye is at rest, seem to draw closer at D and F, are found not to do so when the eye is carried from CE to DF; and a singular effect of motion is produced, the lines CD and EF seeming to move apart as the eye runs down them towards DF. The other lines seem to move correspondingly, and thus we have an illusion of motion, which will be considered later among illusions of that sort.

In the illusory figures now to be considered, the lines which appear curved, though straight, are set in different positions so as to be differently affected, and thus we have illusions affecting the shape of enclosed figures such as shall be considered separately further on.

If we look at Fig. 8 as it stands, that is with the point A uppermost, we find the lines AB, AE looking strongly curved; BC and ED rather less curved; and DC looks almost straight—at least, this is the effect to my eye. (I find EA most curved of all.) Thus the figure, which is

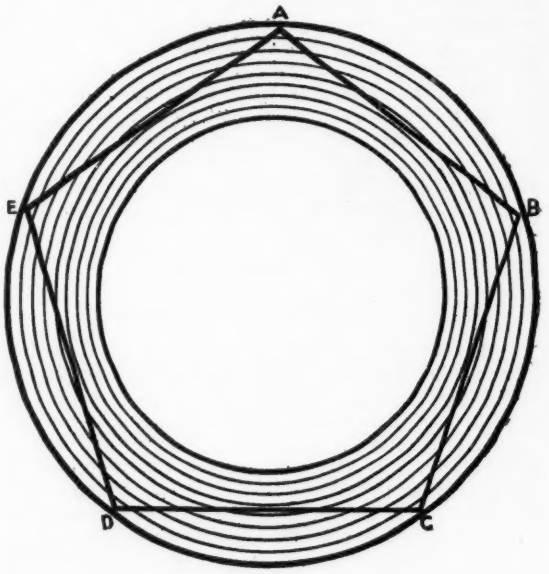


Fig. 8.

really a true pentagon with straight sides, looks like a figure having unequally curved sides. Turning the picture round in its own plane, we find that as the sides of the pentagon by this turning motion pass to the top, they appear more curved, so that they appear to change in shape as they move, or to have a motion of their own.

The apparent curvature of the really straight lines in

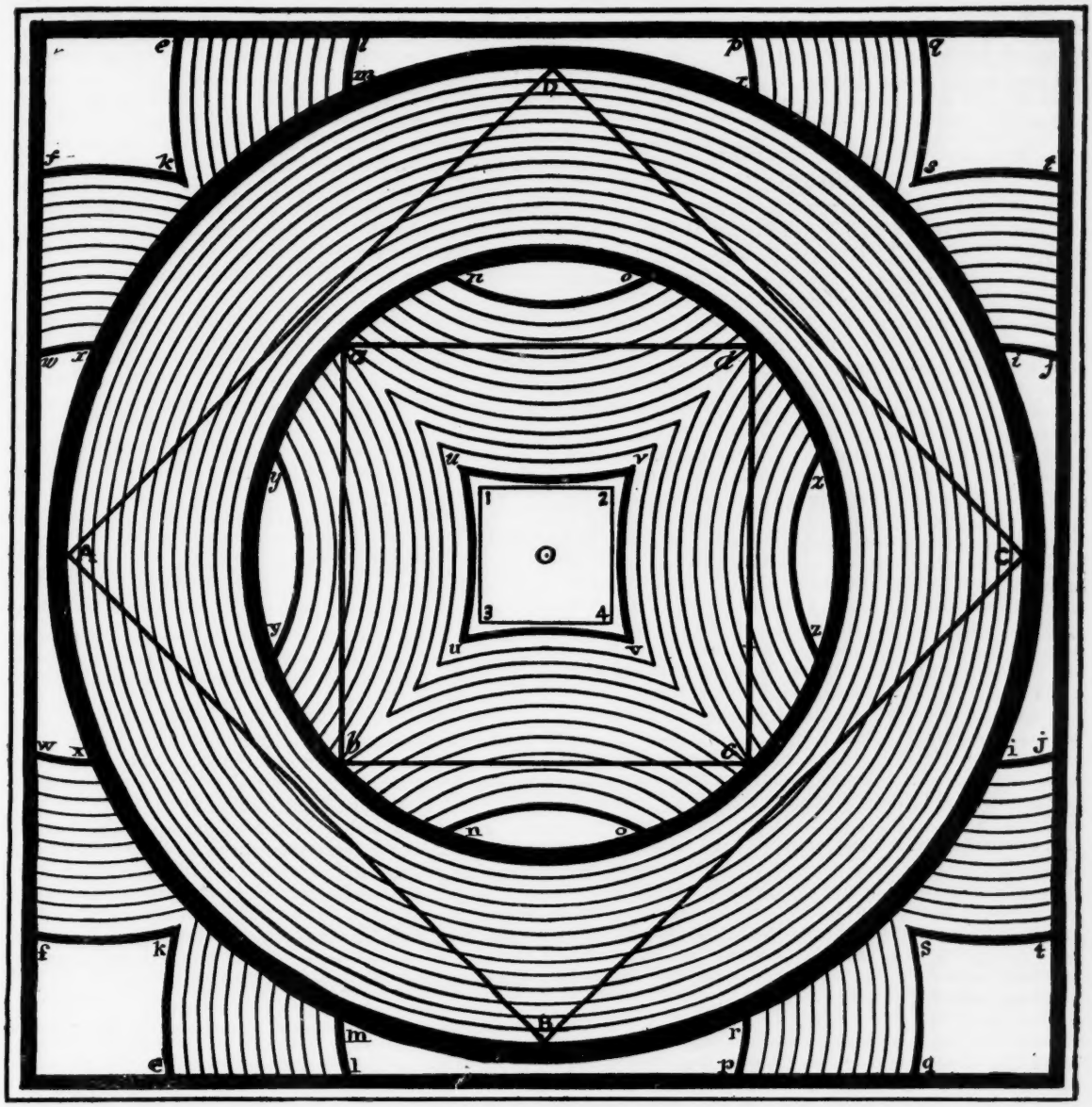


Fig. 9.—A Nest of Illusions.

Fig. 8 has been regarded by Mr. Proctor as having possibly some bearing on the irregular shape, which the outline of the planet sometimes seems to have. It is referred to by Mr. Webb in his "Celestial Objects for Common Telescopes," who illustrates it, however, by a figure in which the distinctive characteristic is lost; the straight line in his figure does not cross the curved lines, and the suggestion of curvature, if it exist at all, is too slight to be readily recognised.

The effect of position on illusions of this sort can hardly, I think, be better illustrated than it is in Fig. 9, which may be described as "a nest of illusions."

Thus, First, the sides of the square ABCD, which are really straight, appear to be curved inwards.

Secondly, the sides of the square *abcd*, which are really straight, appear to be curved outwards, as (in less degree) do the sides of the square 1234.

Thirdly, the space between the two large heavy circles *mir* and *nzn*, appears to be superposed on the rest of the figure.

Fourthly, arc *no* (lighter curve) appears to belong to a smaller circle, instead of belonging, as it really does, to the same circle as the arcs *lm* and *pr*, the same holding with the other corresponding circular arcs of the figure.

Fifthly, it is difficult to realise that the space between the arcs *st* and *ij* is as broad as that between the arcs *vv* and *zz*, &c.

Sixthly, the circles between *no* and *zz* appear to draw closer together, though in reality concentric, as they approach the heavy circle *noz*, &c.

Seventhly, the same circles appear to draw further apart along the region 2*vd*, &c.

But it will be found that the illusion varies in amount for corresponding parts of different portions of the figure; and that, as in the simpler case illustrated in Fig. 8, the different parts of the figure seem to vary in shape as the picture is turned round.

Reviews.

AUTHORS AND PUBLISHERS.*

THIS work, we are told in the preface, is intended chiefly for those who for the first time are about to commit their literary productions to the press, and who are unacquainted with the prevailing practices in regard to printing and publication. "The information contained in it will, no doubt, be more or less familiar to experienced authors." This may be so, but all the same, it is a work which every author, whether experienced or not, should undoubtedly possess. And, unless we mistake, it is a work in which the general public will find much to interest them. It is described as a rudimentary handbook, giving just those particulars concerning paper, printing, binding, and publishing, the preparation of copy, the correction of proofs, the embellishment and illustration of books, and the relations of publishers and their clients, &c., with which an author requires to be acquainted. The publishers have endeavoured to touch upon every point which is likely to arise between the period of the preparation of the manuscript for the press and the actual publication of the book, presenting at the same time a precise and accurate account of the mechanical details of printing. There are also most valuable notes on advertising, reviewing, and the law of copyright.

I am myself tolerably familiar with the matters dealt with in this work. At least, I have written nearly forty works, which have been published for me by five or six firms, with all of which I have had pleasant relations. Yet even for so old a hand as myself, the work before me is full of interest. When I began to write, it would have been still more valuable to me. It would have taught me one lesson, at any rate, which many have only learned by a rather dear experience. Publishers have made mistakes, we know; but, as a rule, the advice of an experienced publisher should be followed by a comparatively inexperienced author, and it should always be carefully weighed. I well remember how thoroughly mistaken I supposed Messrs. Longmans to be when they advised me to print no more than a thousand copies of my first work—"Saturn and its System." I had put so much hard work into that treatise, had filled so many pages with long and complex calculations, had drawn the illustrations so elaborately, that I thought many thousands must needs care for my book, and buy it if it were not too dear; while if but a thousand copies were printed, it must of necessity be rather dear. When they told me that very few cared for formulæ and diagrams, for the "great inequality" of Jupiter and Saturn, and so forth, I thought they underestimated the intelligence of the general reader. That was sixteen years ago, and the first edition of "Saturn and its System" is only just approaching exhaustion.† They knew, and I did not, what was best and wisest.

The advice of publishers about advertising, selection of journals to which a book should be sent for review, and other matters of that sort, is nearly always sound, and is always based on sound considerations. When advertising is left entirely to publishers, they are apt, in some cases, to be a little extravagant, so far as my experience goes. At least, I have found two books, separately published by one firm, selling no better than two precisely similar books, published under similar conditions at another time, by another firm, which were much less expensively advertised. But the selection among so many serials as exist, of those in which a publication should be advertised, is so difficult, and requires so much experience, that the author does wisely to avail himself of his publishers' advice in this respect.

The choice of a title is a point which authors would not care to leave entirely to their publishers; yet more than one of my own books bear titles which were either invented by the publishers, or modified at their suggestion. In the work before us, many curious illustrations of authors' mistakes and weaknesses on this point are given. The most curious, perhaps, are the titles selected by Puritan writers, whose title-pages exhibit such eccentricities as these:—

"Eggs of Charity, layed by the Chickens of the Covenant, and boiled in the Water of Divine Love. Take ye and eat."

"Some fine Biskets baked in the oven of Charity, carefully conserved for the Chickens of the Church, the Sparrows of the Spirit, and the Sweet Swallows of Salvation."

"A Reaping Hook, well tempered for the Ears of the Coming Crop."

"Hooks and Eyes for Believers' Breeches." (May we infer, by the way, that in the seventeenth century hooks and eyes were used "in this connection"—that is, where buttons and button-holes are used in our time?)

"High-heeled Shoes for Dwarfs in Holiness."

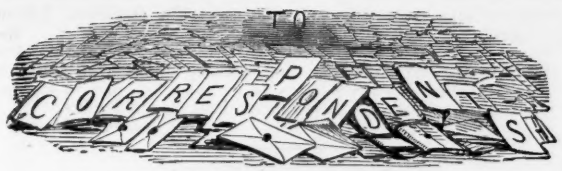
The matter relating to choice of paper, sizes of type, and corrections (this last especially) should be carefully studied by all who write or intend to write books. Others will find it interesting and instructive.

"Authorship and Publication" is eminently readable throughout. Technicalities are avoided where possible, and explained where they cannot be avoided. There is an amusing collection of technical expressions for the various names used to define correctly different degrees of anonymity in authorship, the student of which will be able thenceforward to distinguish an *allonym* from an *anonym*, and a *cryptonym* from a *boustrophedon*.

IMITATION FLOWERS MADE WITH LIQUID FILMS.—A pretty experiment has been recently described by the well-known Belgian physicist, M. Plateau. He bends fine iron wire so as to present the contour of a flower of six petals. The central ring, to which the petals are attached, is supported on a forking stem, which is stuck in a piece of wood. After oxidising the wire slightly with weak nitric acid, the flower is dipped in glyceric liquid, so as to receive films in the petals and the central part. It is then turned up, placed on a table near a window, and covered with a bell jar. For a little while it appears colourless, but soon a striking play of colours commences. In the experiment M. Plateau describes, the flower continued showing modifications of colours for ten hours, when dusk stopped observation. Next morning several petals had burst. The liquid used was of very mediocre quality. M. Plateau recommends preparation of the liquid thus:—Dissolve a fresh piece of Marseilles soap, cut up into small pieces, in 40 parts by weight of hot distilled water. Filter after cooling, and mix thoroughly three volumes of the solution with two of Price's glycerine. The solution should be left at rest till all air bubbles are gone.—*The Times*.

* "Authorship and Publication: A Concise Guide to Authors in matters relating to Printing and Publishing, including the Law of Copyright and a Bibliographical Appendix." (London: Wyman & Sons.)

† I was gratified to learn a few years since, from a criticism of Mr. Geo. Holden (then of the Washington Observatory, now of Ann Arbor), in which he rather severely denounced a book of mine, which is still in manuscript and was then unwritten, that the sales of my first book had been so great as to tempt me to seek a fortune by writing scientific treatises. Otherwise I should have thought the book had involved a rather heavy loss. But we should always believe what we see in print.



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. He requests that all communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All communications should be addressed to the Editor of KNOWLEDGE, *74, Great Queen-street, W.C.*

All Cheques and Post-Office Orders to be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

TO OUR READERS.—NOTICE TO CORRESPONDENTS.—THE SUN'S HEAT.—WASTE OF ENERGY.—THE EARTH'S REVOLUTION.—THE FIFTEEN PUZZLE.—FLIGHT OF BIRDS.—CORRECTED SUN-DIAL.

We begin to see more clearly than we did at first the lines on which our Correspondence columns will have to be conducted, though we shall always be glad to have hints and suggestions from our readers towards the improvement of this section of KNOWLEDGE. Dropping the editorial "we," let me note that from the beginning I have proposed to take my share in the Correspondence, not only in answering queries, explaining difficulties, &c., in subjects I am able to deal with, but also in asking questions, inviting explanations, and so forth. It will be found, also (indeed, I think the last and present numbers give some illustration of the fact), that space will as readily be found for corrections of the mistakes I make myself, as for those into which others may fall. In the two first numbers I appended, in several cases, my replies to the respective letters to which they related; but hereafter I propose to reply each week to letters and queries falling into my department which have appeared in the previous week. Others have promised me their assistance in dealing similarly with other inquiries. But I hope many who have information to give on subjects of interest respecting which inquiries may be made, will do so, as occasion may suggest. We wish to leave as few inquiries unanswered as possible.

I take this opportunity of reminding correspondents that their letters should reach the office by Saturday at latest, if they are to appear in the number for the following week. Already, owing to the unexpectedly rapid growth of the correspondence, it has been found necessary to arrange that no letters received after Monday shall be even read till the matter for the following week has been made up; and, of course, in making room for letters, the rule "first come, first served" must be attended to, not indeed with absolute stringency, but in the main. We must also beg our correspondents to bear with us if we are obliged to abridge some letters. There shall be no favouritism, either in selection, or in inserting more or less from letters which reach us; in every case we shall be guided by our view as to the wishes and requirements of our readers. Nor need correspondents be surprised, any more than we are, if every letter they may send us, or every part of each letter, should not be thought precisely what would suit the plan and purpose of KNOWLEDGE.

Taking first letter 2, p. 15, I note that "Anti-Guebre's" difficulty is one which I have found, during my lecture tours in Great Britain, America, and Australasia, to be more common than any other whatsoever. I suppose I must have received more nearly a thousand than five hundred letters presenting precisely "Anti-Guebre's" difficulty, so that, I may remark in passing, Mr. Newton Crosland must not regard himself as sole inventor and patentee of the paradox in question. The answer is not so simple as many seem to suppose. None of our correspondents have, indeed, answered "Anti-Guebre's" difficulty correctly and fully. If we remember that the existence of snow in large quantities on mountain tops and of ice particles in the cirrus clouds, implies the prior existence at those heights of large quantities of the vapour of water, we shall see that the asserted dryness of the higher air (and, therefore, the unimpeded passage of heat rays through such air) can hardly be regarded as demonstrated. Again, to say that rarefied air has no capacity for heat is, in reality, to speak in terms belonging to the old and erroneous theory of heat as a sort of fluid. Many seem to imagine that the mere rarefaction of a gas is a cooling process, overlooking the statement which accompanies all correct accounts of experiments on the subject, that it is not the rarefaction itself, but the work done in expansion that causes loss of heat. In like manner, when gas is compressed, it is not heated because of its greater density, but because a considerable part of the extraneous work applied to produce compression is transformed into heat. In one case we have force obtained at the expense of so much heat, in the other heat is produced by the expenditure of such and such force. If the air around lofty mountain tops were simply very thin, and there were never any interchange between the higher and lower air, not a particle of snow would ever be seen on our mountain tops, nor should we ever see the cirrus or feathery snow clouds in the upper air. But because there is interchange, because the air which rises along mountain slopes expands and does work in expanding, it is made cooler and cooler, till at great heights it becomes altogether refrigerated, while the air which descends to replace the air which has risen becomes warmer, because in descending it is compressed by the action of gravity (an extraneous force) and a portion of the force thus exerted appears in the form of heat. Again, in the case of the upper feathery snow-clouds, the refrigeration comes mainly from the ascent of aqueous vapour. The wool-pack clouds which we see on a summer day are the upper parts of columns of ascending aqueous vapour, where the vapour has been condensed to water particles or vesicles, and so appears in the form of visible cloud. At the upper surface of these clouds a process of evaporation is continually taking place, and currents of vaporous air are continually ascending. When these ascend high enough to be sufficiently refrigerated, they form in turn into water-drops or vesicles, but under different circumstances from those prevailing where the cumulus or wool-pack clouds are formed. The refrigeration is more rapid owing to the rarity and *relative* dryness of the higher air, and thus the water particles up there form snow-crystals, and (under the conditions stated) cirrus clouds are formed. That they are not always formed is due to the circumstance that these conditions do not always prevail.

As to the waste of solar and stellar energies referred to in the same letter, it is probably only the limited nature of our knowledge which causes that to seem like waste which may, for aught we know, involve the most perfect adaptation possible of the energies in question. We may say of such inquiries, with the poet:—

> "Reason, alas! it does not know itself;
> Yet man, vain man! would with this tiny plummet
> Measure the deep profound.
> He sees but part of the chain—the nearest links—
> His eye not reaching to the equal beam
> That poises all, above."

We need not infer that what we see is seen incorrectly because we see not all.

In letter 5, p. 35, Mr. Crosland uses arguments which only require to be understood to be refuted. What meaning can be found in the statement that "light and heat are phenomenal products, caused by magnetic and electric forces, in a state of intense activity, acting upon atmospheric conditions"? Why not equally well say "Magnetism and electricity are phenomenal products, caused by luminous and calorific forces, acting under aqueous conditions"? Again: the sun might "possess the power of producing the phenomena of incandescence, without being itself incandescent." But, as a matter of fact, he is incandescent, if incandescence means, as it does, glowing. Electricity produced in another room may make a wire on the table at which I write white hot; but when looking at the wire I see it to be glowing white, and when by actual tests I find it to be radiating heat, I cannot agree that it is neither incandescent nor hot, because something which is neither glowing nor a source of radiant heat may yet produce both light and heat.

Several correspondents think "Tyro's" letter (6, p. 35) should not have appeared. It is not very pertinent, but it serves to show one chief way in which paradoxes arise, viz., from want of thought. "Tyro" must have seen hundreds of cases where a luminous body causes a distant opaque body to appear bright, without any flood of effulgence along the space between; yet he expects to see that on the large scale which he does not see on the small scale. I do not take exception to his remarks as relating really to light, not to heat, because I assume he considers that where there is lustre (inherent) there is also heat. It is not always, though it is generally the case.

XUM

"Tyro's" other query (letter 9, p. 36) is one often made, though most text-books sufficiently explain the difficulty. The only kind of revolution which some readers of astronomical works seem capable of understanding is such as we see where a ball at the end of a rigid rod is swayed by the rod round a centre. The real revolution of the earth more nearly resembles the motion of a spinning top when thrown (on a curved path) through the air, the axis moving all the time parallel to itself, or retaining an unchanged direction while changing constantly in place.

In my remarks on letter 13, p. 37, the words ten millions, last line but two of second paragraph, should be ten billions. There are in all more than twenty millions of millions of possible positions in the "fifteen puzzle," though some, overlooking the circumstance that every change in the position of the blank square changes the puzzle, suppose there are only about 1½ millions of millions.

As regards query 3, p. 30, on the flight of birds, it is quite certain, as others, I see, have pointed out, that the air in the passages of the bones cannot help the bird by adding to its buoyancy. If an absolute vacuum could be produced in these passages, which (if the passages remained unchanged in volume) would give the maximum degree of buoyancy, the only lifting power which would result would be simply equivalent to the weight of so much air at the actual temperature of the air in which the bird is flying at the moment, as would fill the passages in the bones. Air equal in volume to the bird itself would not have more than a 400th part of his weight; how much less, then, would the counterpoising of his weight by a raising power corresponding to the weight of the tiny quantity of air which would fill the passages, be of any appreciable avail in helping him to fly?

I will describe and sketch in an early number a very simple instrument for telling time from a shadow (as in the case of the sun-dial), without any appreciable error arising from the shadow not being sharp. RICHARD A. PROCTOR.

THE MISSING LINK.

[33]—I am glad to see a correspondent ("An Ignoramus") has asked for an explanation regarding the "Missing Link" of Darwinism and Evolution. Briefly stated, here is the problem. If, as evolution postulates, the various forms of animal life have "sprung from one, or, at most, a few, primitive forms, then we must conceive of living nature as a tree of which all the parts are connected together from root to topmost twig. Now, if man exists, as he unquestionably does, at the top of the highest twig, it is evident he must be connected, by some forms more or less like himself, with lower quadrupeds, and through these latter, with still lower animals, and so on. Where, then, in the case of man, are the animal links that lead from the human to the pre-human, and from the pre-human to the purely animal? When evolution was first promulgated, everybody asked "Where are the missing links?" That common ignorance, which too often passes for common sense or for science, at once inquired where was the link that connected man with the monkey? This question is founded on gross ignorance of what evolution requires. No evolutionist assumes that man is descended *from any existing ape, or from any extinct ape either.* Mr. Darwin, in his "Descent of Man," is very careful to point this out. What evolution does say is, that probably man and apes originated far back in some common root-stock, whence the human branch proceeded, diverging for ever, from the ape branch, on its own way of development. If we take the four highest apes—gorilla, chimpanzee, orang, and gibbon—we find no one of the four to approach man as a whole more nearly than any other of the four. If an approach to the human frame were to be made from the ape-side, we should require a bit from each and all of the four to make up such a representation of the human type. The orang's brain is, for instance, more like man's than the gorilla's, but the latter approaches man's more nearly in some other points. It is the same with the gibbon and the chimpanzee. The "old ladies of both sexes," who used to assume that the gorilla as the "missing link" was a failure, did not know that the evolutionist thoroughly agreed with them. Where the "links" that connect man to his lower neighbours are to be found, is a difficult question to answer. Possibly we must go first to lower human life, and find an approach to animal characters in the skulls of savages and primitive men (*e.g.*, Neanderthal skull); but the geological record is imperfect. There are long gaps in the series which can never be filled. All living forms have not been preserved in the fossil series. With human remains, the chances of preservation are few and far between. Even primitive man buries or burns his dead; and thus the record of man's past history may ever remain obscure. But all the evidence points indubitably to man's origin from lower life. His development shows this idea to be true; the presence of rudimentary organs (such as ear-muscles, which are of no use to him) tells the same tale; and anthropology, in its researches into savage life and customs, verifies evolution. I hope "Ignoramus" will feel satisfied with the above answer. He should read Darwin's "Descent of Man" and Mr. Tylor's "Anthropology," and an article on "Missing Links," in a recent volume of the *Gentleman's Magazine*, will also help him in his endeavour to understand what evolution demands and implies.

ANDREW WILSON.

THE SUN'S HEAT.

[34]—I must confess I was somewhat surprised to read "Anti-Guebre's" letter in your first issue, considering the preceding remarks about "paradoxers;" but since he asks for information, I shall be glad to give it him.

In the first place, the air is not capable of being directly warmed by the rays of the sun, but it is warmed by contact with the heated earth; consequently the air is much warmer in the valleys than on the tops of the mountains. Again, in the valleys the air is not so subject to disturbance by winds, &c., as it is at a higher elevation; so when we ascend a mountain or rise in a balloon, we recede both from the body of the earth and from the heat thereof, for air is a very bad conductor of heat; but though the *air* is colder, the *sun's rays* are not. If "Anti-Guebre" had ever ascended a snow-capped mountain, he would know that the rays of the sun are insupportably hot there: for two reasons; first, the traveller is *nearer* the sun, and unprotected by clouds and aqueous vapour; secondly, he is exposed to the reflection from the snow. The snow protects the mountain itself from being much warmed, and is only very slowly melted, as it reflects the greater part both of the heat and light. The deposition during the night makes up for the loss experienced during the day. Alpine climbers usually complain that their lower limbs are nearly frozen by the snow, while their heads and shoulders are almost roasted by the sun.

Aqueous vapour plays a very important part in the warming of air, for it is the vapour rather than the air itself that receives the heat. Dry air cannot be warmed so easily as damp air. This is the reason why the air feels warm in dry, frosty weather, as it cannot conduct away the heat from the body; whereas, damp air feels cold, since the aqueous vapour *does* conduct away the animal warmth of our bodies. (Of course, I am not considering the subject of evaporation.) At high elevations the air is colder and dryer than at the surface of the earth.

With regard to the icy nature of cirrus clouds, there is nothing absurd in the supposition.

Anyone who watches the sky in summer will see clouds disappear and reform, often with great rapidity. This is due to the varying temperature of the air, or rather of the air currents, which either condense or volatilise the aqueous vapour in the air, according to their temperature. We know that certain clouds *do* consist of icy particles when we see a halo round the moon making a particular angle with the observer's eye. If "Anti-Guebre" thinks an ice-cloud ought to be instantaneously melted by the heat of the sun, he is in error, as, putting on one side the diathermancy of ice, and granting that the action of the sun would be to melt it, the evaporation at that altitude would be so rapid as to freeze again the water formed, consequently, the volatilisation of the cloud would take some time.

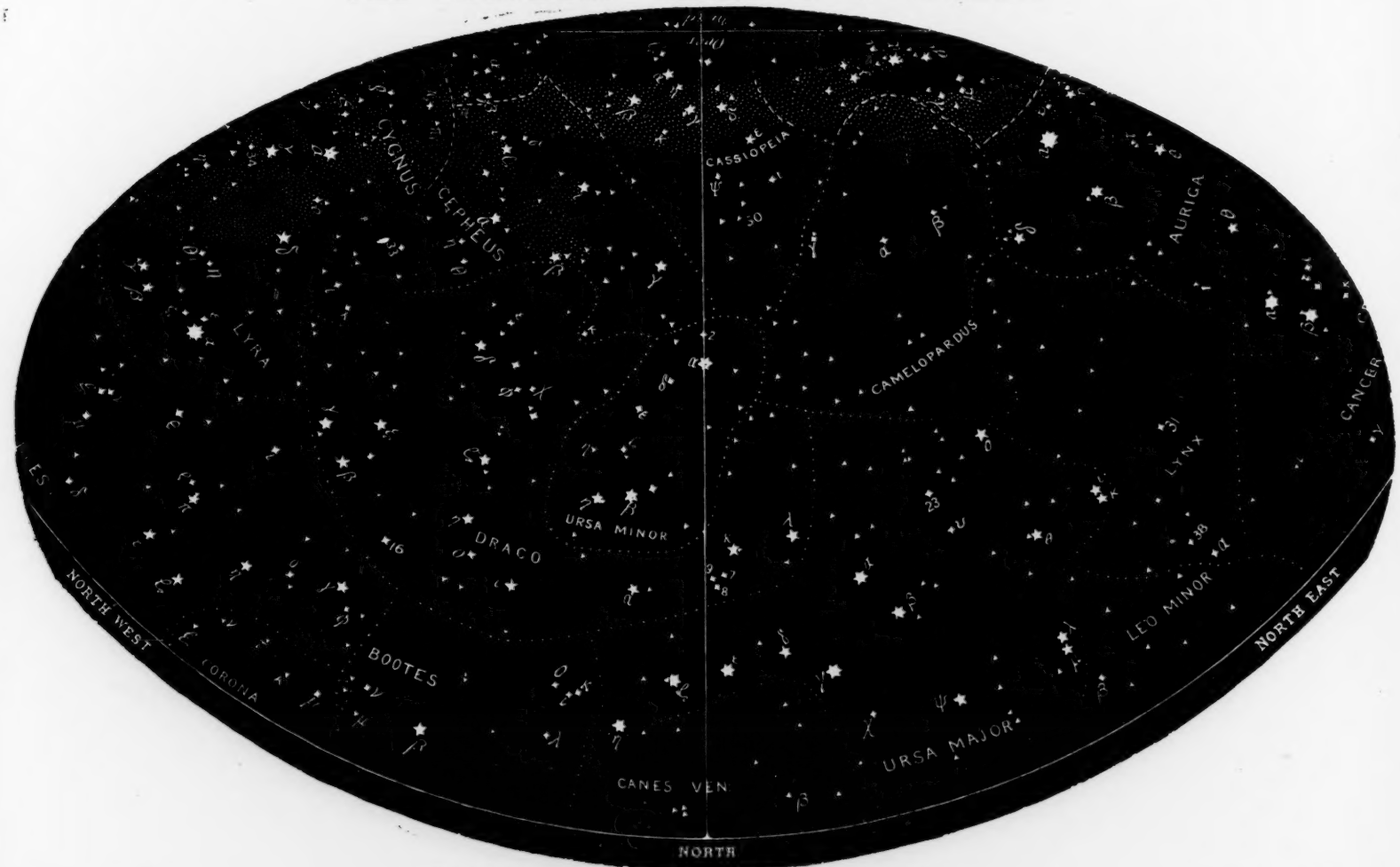
"Anti-Guebre's" "profit and loss" ideas of the solar system are too deep for me. It is true that we only receive $\frac{1}{2,070,650,000}$ of the sun's light and heat; but what right has he to say that the rest is wasted? He must first find out what becomes of it, and prove that it does no work. He might, with equal truth, say his own time is wasted when in bed, because he is not conscious of what takes place when he is asleep.—I am, &c., SM.

THE SUN'S HEAT (*Abstract*).

[35]—"Anti-Guebre" says that when he approaches the fire he feels warmer, but that, on the contrary, an approach to the sun produces a cooling effect, hence the sun is cold. In the case of the fire we make a very great difference in the absolute [it should be relative] distance between us and it, by a very small movement on our part; whilst in the case of the sun, any difference in the absolute [relative] distance between us and that orb which we are able to make is immeasurably small compared with the absolute distance; and though it may be said there *would* be an *augmentation*, though small, there are other causes at work which more than neutralise this augmentation.

It has, I think, been fully proved that the air itself is transparent for heat, and that the air is *warmed* by contact with the earth; the higher, therefore, we rise above the sea-level, the colder does the air become, and this effect is ample to overcome any slight increase in the heating power, owing to a decrease in the direct distance of

THE NORTHERN SKIES IN NOVEMBER.



This Map shows the position of the stars in the Northern Skies:—

On November 3, at 11½ o'clock. | On November 10, at 10½ o'clock. | On November 18, at 10¼ o'clock. | On November 25, at 9½ o'clock. | On December 3, at 9¼ o'clock.
On November 7, at 11 o'clock. | On November 14, at 10¼ o'clock. | On November 22, at 10 o'clock. | On November 29, at 9¼ o'clock. | On December 7, at 9 o'clock.

The stars in the northern skies are carried from left to right under the pole, and from right to left above the pole, descending to the left or west of the pole, and ascending to the right or east of the pole. They are carried, in fact, round the pole in a direction contrary to that in which the hands of a watch move.

*** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.

the sun. It seems to me that the matter is of far greater complexity than your correspondent imagines.—Yours, &c., VERAX.

[36]—Your correspondent "Anti-Guebre" (p 15, No. 1) takes up rather a large amount of what I expect in future will be very valuable space, with his question as to whether the sun is hot, and in proof that it is not so, he brings forward the apparent fact that as we ascend high mountains we get cooler. This, however, is not exactly a fact, and it is very difficult to convince the unscientific public that it is not so, as I found to my trouble during a twenty years' residence as a medical man in India. Now the amount of heat which we are sensible of on a hot day does not come to the body directly or entirely from the sun, but from the earth, which has been heated by the sun's rays. In the hills (I refer more especially to tropical countries), the heat received by the soil during the day is radiated to such an extent during the night, that the surface becomes thoroughly cooled down. In the plains below, probably from the greater density of the superjacent air, the radiation is very small, and the heat is retained, to be added to by the succeeding day's sun. The movement of currents of air, also, is much more constant in the hills, and this acts like a fan in continually changing the air which has become heated by contact with the surface of the earth. The heat received directly from the sun should, of course, increase the higher we ascend, not because we are a few thousand feet nearer to it, but because so much less of its power is absorbed by the lower and denser layers of the atmosphere. And so it does, as is proved by the fact, not so generally known, that the black bulb thermometer indicates the increased temperature as the elevation increases. The readings of this at Ootacamund, in South India, average about 14° above those taken simultaneously at the coast towns. I have repeatedly noticed that Europeans who come from the low country up to the hill sanitaria, while enjoying the cool atmosphere, yet had their faces completely peeled by the increased direct action of the sun's rays. I have seen a child's hat blown off, and before it could be picked up again the child was stricken down by sunstroke, and this at a time when there had been ice on the plain during the night, and I have always noticed that while it was possible to walk short distances in the sun in the low country with an ordinary green silk umbrella, yet that at six or seven thousand feet elevation this became quite insufficient, the heat seeming to strike right through it, at once rendering the addition of a white calico covering absolutely imperative. B. M., F.R.C.S.

Nov. 18.

[37]—In letter 2, page 15, your correspondent says that "the sun is not himself hot; but, of course, he is the source of heat to the earth." How does he reconcile the two statements? He might as well maintain that his fire, which he uses as a conclusive proof to himself of the non-incandescence of the sun, is not in itself hot, though it is a source of heat to things surounding it. He also uses that question which has been so often advanced by inquirers, and as often replied to by scientists, Why is it that the higher you go from the earth the colder it becomes, if, as people say, the sun be hot? He forgets the important part the atmosphere plays with regard to our earth. I maintain that the nearer the earth is to the sun, not the nearer we get up through our atmosphere to it, so the heat transmitted becomes greater. When the earth is at its perigee, the sun transmits more warmth than when it is at its apogee. To this he may advance the argument that the perigee occurs in January. So be it. From this very cause winters are milder and summers cooler in the northern than in the southern hemisphere. Then, as he says the sun is not hot, can he explain why the earth should derive more heat from the sun when its rays fall vertically than when they fall obliquely? If the source of heat be contained in the earth itself, then should the poles be warmer than the equator. We know the opposite to be the case. In Letter 5, page 35, your correspondent says, "Light and heat are surely phenominal products, caused by magnetic and electrical forces in a state of intense activity." If so, why is there greater heat when the sun's rays are vertical, if it has nothing to do with the production of heat? In this letter there is more dealing with probabilities than with facts. Probabilities do not exist in Nature, and I trust that through your columns we may arrive at a clearer idea of the truth concerning this question of the sun's heat. I hardly see that Letter 6 requires an answer, as this is not so much a question of light as of heat.—Yours, very faithfully, G. G. D.

Nov 15, 1881.

POND'S EXTRACT is a certain cure for Rheumatism and Gout.
Pond's Extract is a certain cure for Hæmorrhoids (Piles).
Pond's Extract is a certain cure for Neuralgic pains.
Pond's Extract will heal Burns and Wounds.
Pond's Extract will cure Sprains and Bruises.
Sold by all Chemists. Get the genuine. [ADVT.

STAR NAMES.—COMETS' TAILS.

[38]—May I suggest, in regard to your excellent star maps, that it might be some improvement to them if the names of well-known stars, such as Capella, Aldebaran, &c., were marked in the map, as well as the name of the constellation and the α, β, γ, &c.; or, at least, attention might suitably be called to them in a note naming the brightest star in each particular map.

I have annotated your first map for the benefit of my boys, writing at the side—

Aldebaran = α Tauri,
Rigel = β Orionis, &c., &c.

No explanation I have yet heard as to the material, &c., of comet's tails appears very satisfactory. Has the idea ever occurred to you that a comet repels matter from its nucleus in the form of vapour, or a highly-attenuated condition of solid particles, not in one direction only, but *laterally* as well? And that we might only see the portion of this matter through which a ray of light was projected from the sun or from the glowing mass of the comet itself, at such an angle as to render its path visible to us in the same manner as we see sun-rays at sunrise and sunset, or when the sun is emerging from a cloud?

Would not this account for the apparently incredible rapidity with which comets' tails swing round the sun at perihelion, and also for comets, when distant from the sun, not exhibiting any tail at all?

There are other points I wished to mention, but have not time to-night. I heartily wish your excellent paper every success, and am, yours faithfully, SPECULUM.

PRACTICAL WORK WITH THE TELESCOPE.—MAP OF EASTERN SKIES.

[39]—I am very much pleased with the first two numbers of KNOWLEDGE, and I am doing all I can to make it known amongst friends. I have had some difficulty in getting the numbers, but perhaps this will be all right hereafter.

I have been a "star-gazer" for a few years, and have read a good many of the treatises on astronomy, but have never been able to turn my knowledge into any practical shape. If you could throw out a few hints as a guide for those desiring to start in this study, I think they would be of great service. What particular branch would you recommend to start with, and how ought the study to be carried out? What handbooks and instruments would you recommend?

Surely there is some mistake in your map of the eastern sky issued with No. 1. In your map you place Saturn highest above the horizon, then Jupiter, then the Pleiades; whereas the reverse is the order, Orion is placed very much on its side, and so on. Wishing you every success, I remain, yours, &c.,

Edinburgh, *Nov.* 16, 1881. G. M.

[With reference to the last question, has "G. M." understood that the circular outline of the map is the horizon? Thus it must be held with the word SOUTH-EAST lowest to show the position of the objects he names, which all lie, speaking generally, towards the south-east at the hour corresponding to the map. Note, however, that by an unfortunate error, corrected in part of the present issue, the hours named under the maps in Nos. 1, 2, and 3 are all, without exception, one hour too late. This should be corrected in each map, as the maps, apart from the planets shown, will be as correct for next year, or for twenty years hence, at the same dates, as for the present year.—ED.]

CELESTIAL OBJECTS.

[40]—I have obtained the first two Nos. of KNOWLEDGE, and am much pleased with the contents; the star maps especially I was very pleased to see. Would it be in accordance with your plans to publish, from time to time, a few notes descriptive of interesting and remarkable objects in the constellations? Several of my friends to whom I have shown KNOWLEDGE have promised to become subscribers; and to those of us who dabble a little in astronomy, the name of the Editor is a sufficient guarantee of the information we shall get on that subject, at any rate. Wishing KNOWLEDGE the success it deserves,—I remain, yours, &c. W. J. C.

ARE WOMEN INFERIOR TO MEN? (*Abstract.*)

[41]—I perused with some interest in the first issue of your paper an article on the question whether women are inferior to men. The study of the human mind and character seems to be neglected by most publications, although it is a most interesting subject. I am sure if you will open your columns for such subjects, you will not fail to gain appreciative and constant readers. The

XUM

subject in question is one of which different views may be taken, and I need therefore make no apology for writing a few words on it. The writer of the article seems to be under the impression that he has only to prove the fallacy of M. Delaunay's arguments to show that woman is not inferior to man. I have no doubt but what such a question will more frequently present itself to the public mind as the competition in the labour markets between men and women becomes keener. The author of the article throws just ridicule on the comparisons made by M. Delaunay, yet he himself does not hint in the least that the comparisons are altogether improper and most unjust to the fair sex, as they really are. Men are too apt to forget that the first duty of woman is to keep a home and rear their young; to unite with, and not struggle against men. Man and women united form a perfect being; they are one in every sense of the word; apart they are both imperfect. Wishing KNOWLEDGE every success, Yours, G. W. D.

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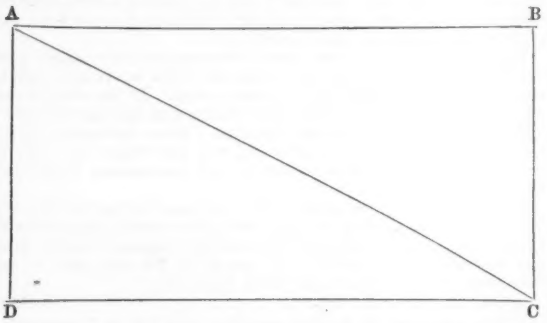
[42]—In your review of the above, in Vol. I., No. 1, p. 8, you say, "We come next, in M. Delaunay's paper, to the more important question of cranial capacity and form; for, certainly (setting aside, of course, phrenological absurdities)," &c. 1. How do you *prove* your "*assumptive statement*" as to phrenology being *absurd, ergo,* false? 2. How do you account for your *special* knowledge relative to capacity and form, apart from phrenology? 3. Does the brain shape or mould the form of the cranium or skull?—Yours truly,

CHARLES HAMILTON.

SPEED OF AMERICAN ICE-YACHTS.

[43]—I think I can show non-mathematical readers a way in which they can satisfy themselves that an ice-yacht *may* sail faster than the wind.



In a board or card cut a groove, or slit from A to C; in it fix a freely moving slider; lay a ruler along A B, and, keeping it parallel to itself, slide it down to CD. The slider represents the yacht, the groove the effect of the yacht's runners, and the ruler the front of an advancing body of wind. As, then, the slider moves faster than the ruler, describing AC, while the latter describes AB, so the yacht may move faster than the wind. Of course, I do not say that the above arrangement explains all the forces in action; but I think it removes the chief difficulty, which seems to be in failing to see how the effect (so to speak) can be greater than the cause.

W. J. M.

[The reader will notice that "W. J. M." carefully limits the application of this reasoning. It shows only—as he mentions—that an ice-yacht *may* move faster than the driving force. Any amount of energy may be applied to move the ruler until the resistance of the slider is overcome. The ruler cannot move as described unless the slider gives way in the only way in which it can move; in the case of the yacht, the wind may blow over and past the yacht. But "W. J. M.'s" experiment shows all he claims for it.—ED.]

ARE MEN'S HEADS SMALLER THAN OF YORE?

[44]—In a letter on "Are Men's Heads Smaller than of Yore" in your Nov. 11 issue, a quotation is given from Professor Flower, in which several suggestions are thrown out as to reasons why hats should be smaller than they were twenty years ago, without a corresponding diminution in the size of heads. As a seller of hats for many years, I am able to verify several of those reasons to account for smaller-sized hats being required. No doubt, the hair being cut closer makes at least half a size difference, but the present manner of wearing hats on the top of the head, instead of down over the back of head and ears, as formerly, would make the most difference (at least half an inch), which any one can prove for themselves by measurement. Again, youths and young men now wear hats instead of caps, so that in sorting up sizes for sale, I find it necessary, if it is a fashionable shape, and likely to be worn by young men, to order—

Sizes	20½	20⅞	21¼	21⅝	22 inches
Relative numbers ...	1	2	3	2	1

while for older styles, just because older men wear their hats further on the head, and like an easy fit, and not because their heads are larger, I have to order—

Sizes	21¼	21⅝	22	22⅜	22¾	23¼ inches
Relative numbers ..	1	2	3	3	2	1

Yours, &c. HATTER.

ARE MEN'S HEADS SMALLER THAN OF YORE?—THE FLAT EARTH THEORY. (See letter 12, p. 36.)

[45]—Has it ever been noticed that most grooms, and such men, wear their hats, generally, on the *side* of the head? Possibly this may have something to do with the supposed change.

The following is from an article on Physical Geography, which appeared in a book published a short time ago:—

"It is remarkable that persistent efforts are still made, and supported by a great display of mathematical technicalities, to prove that it (the earth) is a circular disc; and within the last few years elaborately illustrated books have been produced in advocacy of the supposition."

I should be glad if you would say who are the publishers and what is the price of the books. Or if you could tell me on what facts the writers base their theory.—BETA.

[See reply to John Hampden in Answers to Correspondents.—ED.]

"KNOWLEDGE," TECHNICAL TERMS, SCIENCE, AND RELIGION.

[46]—I have been very much pleased with the first two numbers of KNOWLEDGE. If the forthcoming numbers be as good as Nos. I. and II. promise, you will supply a great want, and deserve an equally great popularity. I have done what little I can to advertise it, by placing it in our school reading-room, and by sending it by post or otherwise to various friends.

I had intended to suggest what "A.T.C." has mentioned, the advisability of printing the technical terms side by side with the more popular modes of expression; by this means a double education would be carried on.

I congratulate you upon your article maintaining the harmony, real if not apparent, of the teachings of science with revelation, and your remarks on the wickedness of neglecting to use the minds God has given us in the study of the "outward and visible signs" of His power. Your article must, I feel sure, have given great pleasure to many whose belief in evolution is no less devout than their belief in revelation. Would that all our religious instructors could act in the spirit in which you intend to conduct KNOWLEDGE. With every wish for your success, which I shall do all in my power to promote,

I am, yours, &c.,

J. JOHNSON HOYLE, B.A.Lond.

[Have omitted remarks relative to some of the questions in No. I., though fully agreeing with you.—ED.]

INFLUENCE OF SEX ON MIND: CRANIAL CONTOUR.

[47]—To say woman's skull is less than man's is ambiguous and misleading. Of course, it is *absolutely* less, otherwise woman's head and body would be disproportionate. I think male and female skulls relatively equal. Artists divide the male figure into eight heads, and the female into seven and a half. Grant, then, the female skull relatively somewhat larger than the male. "The brain diminishes and the nerves increase from man downwards; in the fœtus and child, the nerves are proportionately larger than in the adult." (Lawrence: "Lectures," chapter vi.) Walker states that the nerves connecting the brain with external organs of sense are comparatively larger in woman than in man. The eyes excepted, external organs of sense are smaller in woman than in man. Does anatomy support Walker's statement? If so, it helps to explain observed fundamental differences in male and female intelligence. Woman is perceptive; man reflective. Woman excels in sensibility and observation. Man is a being of the intellect; woman of instinct and emotion. Man reasons and reflects; woman perceives and feels. Man is active; woman passive. To man belongs the kingdom of the head; to woman the empire of the heart. Chamfort epigrammatically says:—"Woman has a cell less in the head, a fibre more in the heart."

The sexes cannot exchange sovereignties. Women is less guided by intellect than by feeling and impulse. Her movements are more

easy and prompt, though less sustained, favoured by ready obedience of muscular action and short stature. She is less combative than man. (This rule has serious exceptions.) She desires to please. Man's mission is to protect and defend. Her disposition to sustain mental and bodily exertion is much less than man's. She is fonder of change, and more fluctuating in opinion. *Varium et mutabile semper fœmina. La donna è mobile qual piuma al vento.* Not by her understanding or mental force, but by her prompt and easily-affected sensibility, is woman eminently adapted to surmount maternal suffering; through affection and pity, to be interested in children and household cares. Where her heart is touched, a woman will make incredible sacrifices for a lover, a husband, a child, a parent. She is constitutionally fitted to be wife and mother, to "guide the house," for minutiæ of details. A girl of sixteen makes a better housekeeper than a man of sixty. Woman is more sedentary than man. Her disposition is milder. She is less addicted to great crimes.

Woman's face resembles the child's in absence of beard, rounded form, smooth skin, and brilliant complexion. The infantile type of head appears in smallness of features relative to skull, and perpendicular forehead. Welcker says, woman displays orthomètopy (perpendicular forehead) and a decided tendency to prognathism. I have seen both combined in the same skull, though a retreating forehead and projecting jaws generally go together; and orthomètopy generally accompanies orthognathism. Camper's facial angle is a very uncertain measure of intelligence; according to it alone, the child would stand higher than the man. "If skulls are ranged according to Camper's angle, the infant's skull, contrasted with any animal's, occupies a higher place than the adult's; but if skulls are ranged according to the increasing angle of the sella, the series stand—man, woman, child, animal." (Welcker.) A front view of head and face is well defined by the outline of an egg. The big end represents the skull, the small end the chin. The egg's short diameter gives the position of the eyes, which bisect the oval. If in man, the part above be greater than that beneath the eyes, it is not objectionable, because we associate this excess of coronal elevation with intellectual qualities characteristic of sex. In woman's head, a similar excess in the superior region is a fault in beauty, for which the masculine qualities corresponding to this head-type do not compensate. Ancient Greek sculptors perfectly understood this important distinction in the cranial contour of the sexes. The female head contrasts well with the lofty, massive square brows of male heads. Of the Venus de Medici, Walker observes:—"The size of the head is sufficiently small to leave that preponderance to the vital organs in the chest, which makes the nutritive system peculiarly that of woman. This is the first and most striking proof of the profound knowledge of the artist, the principles of whose art taught him that the vast head was characteristic of a very different female personage. Phrenologists have told us that the head of the Venus is too small. They might as well have said that the head of Minerva or the Jupiter is too large, or a hundred other ignorant, inapplicable, or ridiculous pedantries" ("Analysis of Beauty"). The Gradgrind utilitarian school depreciate the fine arts. Independently of intellectual pleasure, what actual knowledge in these masterpieces! Ancient sculptors could learn nothing of beautiful forms from phrenologists. The latter may learn much from ancient sculptors of the influence of sex on mind.

Nov. 14. J. McGregor Allan.

SPEED OF ICE-YACHTS.—THE FIFTEEN PUZZLE.—CHEAP TELESCOPE AND MICROSCOPE.—TECHNICAL TERMS RELATING TO TELESCOPES.

[48]—There are two things in last week's Knowledge that I wish to mention to you:—1. In explaining the ice-yacht, you say the parallel wind HG (diagram) will increase the yacht's velocity, *which already exceeds that of the wind.* It seems to me that you do the very thing which you take exception to "Upsilon," doing, viz., take it for granted; but, perhaps, I may have missed your meaning.

The other thing (2) is the "Fifteen Puzzle." I am sure many of your readers would like, as well as myself, to hear more of this puzzle; what "the true won position" means; in fact, to explain what the puzzle means, as I candidly confess I have never heard of it before.

There is another matter I wish you would help me in. For some time past I have wished to become the possessor of a telescope and a microscope, but have not been able to see my way to doing so. I could afford to give about £5 each in purchasing them. With regard to the microscope, my ambition is to take, were it years hence, a degree, both in surgery and medicine. I know that a monocular is best suited for histological work; but I should like the time that I would spend at the microscope to materially serve me afterwards. Which should I purchase, a binocular or monocular? Could I get one to serve my purpose for £5, or would I require to pay more? Then, as for the telescope, I am puzzled at the various technical terms used in the magazines I come across, for instance:—(1), Equatorial telescope; (2), astronomical telescope; (3), terrestrial eyepiece; (4), reflector; (5), refractor; (6), 11-inch achromatic; (7), 6½-inch object-glass; (8), 9½-inch mirror; (9), altazimuth stand; (10), equatorial mountings and divided circles, &c. I shall feel obliged if you will tell me what to purchase and where. I should like one that would do some good work for me, and repay me the cost in knowledge obtained. I may add that I have done my part by getting you another subscriber, and I sincerely wish Knowledge success.—Yours, &c., Twenty.

[The explanation at p. 36 shows that there remains a driving wind whose velocity is represented by HG when the yacht's velocity is represented by CE, or is greater than that of the actual wind FE. *A fortiori*, there is a driving wind for all smaller velocities. Starting from rest under the action of a wind in the quarter represented by FE, the yacht will travel with constantly-increasing velocity until the driving force is just balanced by frictional resistance, and it is shown at p. 36 that even when a velocity exceeding that of the wind has been attained, a driving wind remains, which may be quite sufficient to do more than merely maintain the speed attained. Suppose, for instance, that FE in Fig. 2, p. 36, represents a 40-knot breeze, then HG represents (it will be found, on measurement), a six-knot breeze. Now, an ice-yacht moves freely from rest under a six-knot stern wind, so that the velocity of the ice-yacht under the conditions illustrated in Fig. 2 would still increase, though CE corresponds to a velocity of more than 50 knots per hour.

I supposed every one knew the Fifteen Puzzle. It consists simply of a square space, within which are placed, first, sixteen square blocks, numbered in order from 1 to 16. Block 16 is removed. The rest are placed in any random position within the square space; and the puzzle is, by sliding the blocks successively into the vacant square which remains after each sliding motion, to get them into the order shown in the adjacent figure. A prize is said to have been offered in America to any one who should bring the blocks into this position—called the won position—starting from a position differing only from the "won position" in having the three blocks in the fourth line arranged 13, 15, 14, instead of 13, 14, 15 (a position which has been called the "lost position"), and thousands wasted hours on hours of their time in the attempt to do this impossible thing. Some said they had done it, but were assuredly mistaken. Others thought they had satisfied the conditions of the problem by getting some such arrangement as these:—

1	2	3	4
5	6	7	8
9	10	11	12
13	14	15	

	1	2	3
4	5	6	7
8	9	10	11
12	13	14	15

4	8	12	
3	7	11	15
2	6	10	14
1	5	9	13

But the true won position never can be obtained from the lost position. The problem, however, like squaring the circle, trisecting an angle, duplicating the cube, and finding the perpetual motion, has had a singular charm for many, and especially for those to whom the word impossible is as a red rag to a turkey.

The question relating to telescopes and microscopes I must leave others to answer; I have never possessed a £5 telescope, and have but little idea what an instrument can be made to do at that price.

The technical terms mentioned by "Twenty" are no more mysterious than the terms binocular (for two eyes), and monocular (for one eye), which he uses himself. An astronomical telescope is one which shows objects inverted (avoiding the loss of light which results from use of lenses for making the object appear upright); a terrestrial eye-piece is the tube (next the eye) containing such lenses; it is sometimes called an erecting eye-piece; an equatorial is one which, instead of turning round on an upright axis, and moving upwards and downwards round a horizontal axis, like ordinary terrestrial telescopes, is carried round an axis directed to the pole of the heavens, moving also on another axis, so as to be inclinable at any angle to the polar axis; and so forth. But any guide to the use of the telescope explains these points. We may presently publish in these pages some simple papers on such matters.—Ed.]

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Queries.

[19]—Comparative Anatomy of Birds and Animals.—Can you kindly tell me through your paper the corresponding bones in man and other mammals to the furcula of birds? I have consulted all the books to my hand, and cannot find out. Wishing all success to your interesting paper.—Charles Sherborn.

[20]—Orrery.—Can you or any of your readers kindly inform me where an Orrery can be seen?—Vulcan.

[21]—Deep-Sea Soundings.—Could you tell me where I can find an account of "Deep-Sea Soundings" taken off the coast of New Guinea?—Stanhope T. Speer.

[22]—Fomalhaut.—Is this visible in England at about 9 p.m.? My little daughter having decided, by a study of your star maps, in the affirmative.—Stanhope T. Speer.

[Yes, it is the star *a*, in constellation Piscis Australis, shown near the south-west horizon in map illustration No. 2.—Ed.]

[23]—Faure's Accumulator.—Would the editor oblige a reader of Knowledge by stating how De Fauro's battery for the storage or accumulation of electricity is constructed, or in what publication such an account is to be found?—Nameless.

[24]—Dreams.—What position does science take on the subject of dreams? Does it deny positively that dreams ever have been sent as warnings, or that the warnings which dreams have apparently conveyed have ever been fulfilled except by accident? I have seen it stated that faith in dream warnings is as much out of date now as faith in astrology. Yet many accounts which have been given of dreams which have been apparently fulfilled, seem scarcely to be explained away so lightly.—A Dreamer.

[25]—Forms of Food.—Would not a short article, explaining the meaning of some of the terms used in Dr. Carpenter's interesting article on the "Relation of Food to Muscular Work," be of great use to many whose studies have not yet shown them the real meaning of such words as hydrocarbons, non-nitrogenous, and so forth?—Desdichado.

[26]—Training.—It seems to me that it would follow from Dr. Carpenter's theory of the "Relation of Food to Muscular Work," that the system of training followed by our athletes is unsound. Ought we not to return to the system of the ancients, who trained their athletes on barley cakes and oil?—Oarsman.

[27]—Effects of Marriage on the Death-rate.—I should be glad if the editor of Knowledge, or any reader who can give the desired information, could tell me how far it has been made out, or whether it has been made out, that marriage acts as a preservative. Is the death-rate of the married lower than that of the unmarried?—Benedict.

[28]—Stone on Rolling Wheels.—A long stone is rolled forward on wheels 2ft. in diameter, or say 75in. in circumference. There is no sliding. How far does the stone advance for each revolution of the wheels on the top of which it rests? A mathematical friend says the stone advances 150in.; but I cannot see how it can advance more than 75in.—Queensland.

A New Comparison of Poisons.—Comparative experiments with different poisons have often been made by injecting a given quantity of each into the veins of animals, and noting the effects. M. Richet has recently tried another method (which offers some advantages)—viz., poisoning the medium in which the animal breathes. If a fish be put in a poisonous solution, it dies sooner or later, according to the concentration of the poison. M. Richet adopts as the "limit of toxicity," the *maximum* quantity of poison (referred to one litre of water) allowing a fish to live more than 48 hours. This limit he has determined for various metals, always using the same acid radical—viz., chlorides. The limit of toxicity was calculated, not per weight of chloride, but per weight of combined metal. The figures show that there is no precise relation between the atomic weight of a substance and its poisonous power. Copper is 600 times as poisonous as strontium, though its atomic weight is less. Lithium, with an atomic weight only the twentieth of that of barium, is three times as poisonous, &c. Even with metals of the same family, no relation between the two things was discoverable. Cadmium (112) is only about half as poisonous as zinc (65); lithium (17) is 70 times as poisonous as sodium (23), &c. Nor could any relation be made out between the chemical function of a body and its toxical power. Thus, potassium and sodium, the chemical properties of which are so similar, have very unequal toxicity; one gramme of potassium is nearly 250 times as poisonous as one gramme of sodium. M. Richet means to prosecute the subject further.—*The Times.*

Replies to Queries.

[1]—Ultima Thule.—In answer to Query No. 1 in Knowledge, Nov. 11, "Alpha Sigma" will find "Ultima Thule" mentioned in Virgil's "Georgics," Bk. 1, line 30, in the sense of some remote country.—G. R. F.

[This query is answered in a similar way by many correspondents.]

[1]—Ultima Thule.—The following account is given by Lemprière in his "Classical Dictionary":—"Thule, an island in the most northern parts of the German Ocean, to which, on account of its great distance from the continent, the ancients gave the epithet of *Ultima*. Its situation was never accurately ascertained, hence its present name is unknown by modern historians. Some suppose that it is the island now called Iceland, or part of Greenland, whilst others imagine it to be the Shetland Isles. *Stat.* 3. *Sylv.* 5, v. 20.—*Strab.* 1.—*Mela*, 3, c. 6.—*Tacit. Agric.* 10.—*Plin.* 2, c. 75, 1, 4, c. 16.—*Virg. G.* 1, v. 30.—*Juv.* 15, v. 112.—*Solin.* 20.—*Servius ad Virg. loco. cit.*—W. E. M.

[2]—A Fifteen Puzzle.—It is easier to solve the Fifteen Puzzle than to give the demonstration for which "Rusticus" seeks. The solution is as follows (or at least this is one solution): Let the fifteen girls be called A, B, C, &c., down to O, then the seven arrangements are these—

ABC	ADE	AFG	AHI	AJK	ALM	ANO
DIJ	BFH	BIK	BJL	BMO	BND	BGE
EFL	CKM	CLN	COE	CDF	CGI	CHJ
GKO	GJN	DHO	DGM	EIN	EHK	DKL
HMN	ILO	EJM	FKN	GHL	FJO	FIM

It is very easy for nine girls to go out on four days. The arrangement runs thus—

ABC	ADE	AFG	AHI
DGH	BFH	BDI	BEG
EFI	CGI	CEH	CDF

Mathematicus.

[4]—The Earth's Inclination.—Were the Earth to rotate on an upright axis, the greatest amount of heat would always exist at the equator, while the least would be at the poles. At all points of longitude between, the heat would be in proportion to the distance from the equator. I would express it so: perpetual summer would reign at the equator, as the sun would be always vertical to some point there, and perpetual winter at the poles, as the sun would always be on the horizon.—L. T. F.

[5]—Hot Winds, Cause of.—There is usually an inrush towards hot areas, but at times the action is reversed. There is no reason why at times a sandy region, like the Desert of Sahara, should not become a region of high pressure (especially when we remember the rapid radiation of heat at night), and in that case the flow of air would be from that region to surrounding regions, the air carrying before it the heated air. Again, the heated air which has flowed upwards may descend not far from the region of greatest heat, and travel as a hot wind from the hot, sandy region.—Meteorological.

Nature's Respirator.—The season of cold, raw, damp, and foggy weather is upon us, and many will be its victims. Among other causes, the inhalation of cold, and especially of dirty, air must take a high place. Man is, however, provided with a safeguard against this danger. The double passage to the lungs through the nose and through the mouth suggests some difference in use, and this become certain when we find such a difference in the two routes as actually exists. The air passing into the lungs through the nose in quiet respiration is warmed as it passes over the lower turbinated bone, with its very vascular mucous membrane, while, as the cavity is so narrow, it is also to a great extent filtered, and in this way deprived of its two dangerous characters even before it reaches the larynx. Those, therefore, who in the cold and in the fogs wish to avoid catarrhs should be careful to inspire only through the nose. With most this will require some practice, but it will be well repaid. Some, too, will find the impure air of a London fog very irritating to the nasal mucous membrane, and thus a demonstration of the irritant properties of the suspended matter which in breathing through the mouth gets free access to the lungs may be obtained. Those who are specially anxious to preserve their voice—as preachers, singers, and judges—stand in special need of this precaution, which is as effective as it is simple, and has many and great advantages over all the artificial respirators yet invented.—*Lancet.*

XUM

ANNIVERSARY MEETING OF THE BIRMINGHAM AND MIDLAND INSTITUTE UNION OF TEACHERS AND STUDENTS.

By W. Mattieu Williams.

In the first number of Knowledge I communicated a sketch of the history of the Scientific Department of the Midland Institute, so far as the classes are concerned. But there is another element, viz., the Students' own Associations for Mutual Improvement, which is well worthy of notice and imitation.

One of these, the Institute Scientific Society, has been remarkably successful. It possesses a scientific library of no mean character, and its members read admirable papers and carry on discussions of considerable interest. Some of these papers or lectures on the Birmingham trades, written by practical workers who, at the classes, have attained sufficient scientific knowledge to discuss the philosophy of their daily avocations, supply a kind of information not easily attainable from books or the lectures of ordinary professors.

The Union of Teachers and Students, another and larger society, held its anniversary gathering on Tuesday evening, Nov. 22. The programme included a tea-party, the whole arrangements of which were conducted by the female students, without external aid of contractor or purveyor. This was followed by a meeting in the new theatre, under the presidency of the Mayor, where an address was read by the retiring President of the Union, Mr. C. J. Woodward. The subject was the history of the institute. I must not be tempted to quote any more than the following—viz., that when Mr. Rickard commenced the Penny Arithmetic Classes, he had six pupils to the first lessons. This session the attendance to the first lesson was two hundred, and there are now held every week no less than sixty-five "Penny" classes on different subjects in the central institute and its branches, besides all the other classes.

Then followed a general *conversazione*, distributed through the various class-rooms and lecture-theatres, including an exhibition of microscopes contributed by the members of the Institute Scientific Society, scientific experiments by students of the Chemistry and Physic Classes, vocal music by members of the Singing Classes, German recitations, a French play—"Un Quartier Tranquille"—by members of the Institute French Dramatic Club, with the usual social and loyal conclusion of "Auld Lang Syne" and "God Save the Queen" by everybody.

Criticism would be out of place here, and description of details possibly tedious. I need only add that the whole programme was successfully carried out.

The attendance, which commenced with 450 at the tea-party, grew to above a thousand later in the evening, *i.e.*, after working hours.

The feature to which I wish to direct particular attention is the spontaneous, self-originating, and self-supporting character of these proceedings, and of all the other doings of these student associations. They constitute what appears to me to be a most important adjunct to the classes and public lectures of the Midland Institute, and one which may be very advantageously introduced in other kindred institutions, especially those of London. One of the most shallow and mischievous of popular delusions on the subject of education is the supposing that when we have completed a certain prescribed course of study, and passed our examinations on any subject, we have completed that part of our education—the fact being that all class teaching and all book reading is but the first stage of true, comprehensive education; self-teaching, original thought, the digesting and co-ordination of such school knowledge, must follow, to render it truly fruitful, and social co-operation in such supplementary work is most desirable. The meeting of old students with their younger successors, the revisiting of the old teachers, and sustaining of the old friendships between them and their former pupils, gives vitality and moral warmth to the whole institution, prevents the possibility of that decay which too often falls upon such institutions, when their existence is allowed to depend upon the efforts of outside patrons and the beneficence of mere endowments. Besides all this, the governing body is kept justly informed of the real requirements of the students—those who have good reason to be grateful to it, and know its workings by their own experience as former pupils, remain attached to it, join in its management, and otherwise substantially express their gratitude.

The genuine enthusiasm and hard-working efforts in carrying out the evening's programme, the genial friendship and high moral tone which I witnessed as pervading all the proceedings of Tuesday's meeting, convinces me that if such unions and friendly gatherings of teachers and students, old and young, male and female, should become one of the essential elements of all our literary and scientific institutions, their general prosperity and practical effectiveness would be greatly promoted.

Our Mathematical Column.

PRACTICAL USE OF LOGARITHMIC TABLES.

Let us now take a few examples of the practical use of a table of logarithms, noting that the former paper was intended to explain all that is necessary to be known respecting the theory of logarithms. I did not then think it necessary to draw any distinction between the logarithms of our tables and logarithms to any other base than 10; for the computers who mostly employ logarithms, use the decimal notation.

Let us first take the example afforded by Mr. Harding's calculation at p. 55, noting that the result, corrected for a "printer's error," is

$$= 21{\cdot}9912.\ \frac{143303}{153664}$$

We have to take out the logarithms of these three numbers. Take first 21·9912. We turn to the number 2199 in the table and run our eye to the second column above which is the next digit, 1, getting the logarithm 3422450. (The first three digits of this are shown in the first column, the other columns only giving the next four for each number). But we still have to provide for the last digit, 2. Now we might do this from the part of the tables already used. Thus they show:—

logarithm of 2·1991 is 3422450,
and logarithm of 2·1992 is 3422647, or 197 more.

Now, we see that 2·19912 is only two-tenths of the way from 2·1991 to 2·1992, so that we should add only two-tenths of 197 to the logarithm of 2·1991 to get the logarithm of 2·19912, assuming that the logarithm increases, for such small differences, proportionately with the number of which it is the logarithm—which is shown to be true by the circumstance that we have the difference 197 or 198 (*oftener the latter*) for several logarithms on either side of the one we are using. Manifestly if in passing from 21990 to to 21991 and thence to 21992, 21993, and so forth, we have the same difference*, the logarithm is here growing in the same proportion as the anti-logarithm (that is, as the number of which it is the logarithm). Hence, we take two-tenths of 198 (note italicised words above), or 39·6 (the nearest whole number to which is 40), and add this to 3422450, the logarithm of 2·1991, to get the logarithm of 2·19912. Thus

log. 21·9912 = 1·3422490.

But we are saved even this slight labour by good tables. All tables give the difference as 198 in our example; but in good tables there is shown on the right the table of proportional parts, giving the amount to be added for digits 1, 2, 3, 4, 5, &c., respectively, and opposite 2 is set 40, the amount to be added.

Let us proceed in the same way with 143303 and 153664. We find in the tables, logarithm 1·4330 is 1562462, the "difference" is 303, and 3-10ths of this are 91, which added to 1562462 gives us

log. 143303 = 5·1562552.

Again, we find in the tables, logarithm of 1·5366 is 1865608, "difference" is 283, and 4-10ths of this are 113, which added to 1865608 gives

log. 153664 = 5·1865721

Thus, according to the principles on which logarithms are used, our "sum" is worked thus:—

log. 21·9912 = 1·3422490
log. 143303 = 5·1562552

Sum = 6·4985042
log. 153664 = 5·1865721

Difference = 1·3119321 [= log. 20·508].

We now turn to the tables, and looking first along the left hand column of logarithms (next to the column of numbers) for the part 311 of the logarithm we have found. This comes next the number 2047, but running along this part of the tables for the remaining part, 9321, or what comes nearest to it, we find it opposite 2050 under the ninth column, corresponding to digit 8 (shown at the top of this column). The logarithm given here is 3119233 while that next larger is 3119445; the former is nearest to the logarithm above obtained, 3119321. Thus, if we are content with this degree of approximation to the result we want we write down 20508 as the digits representing that result, but as in the bracketed part of above computation we set the decimal point after the *second* digit, because our logarithm has 1 on the left of the decimal point. As a matter of fact, it would be absurd not to be content with this degree of approximation, simply because we cannot get more out of the

* The same difference, for though the actual difference alternates hereabouts between 197 and 198, this is only due to the circumstance that the last digit has to be the *nearest* to the true value, and cannot represent the exact value.

XUM

numbers we are using. Having 21·9912 correct only to the last place but one (the real number runs 21·991485, &c.) our result cannot be correct to a greater degree of approximation. Therefore, we set down only the third decimal figure.

If, however, our *data* would allow us to take more out of our logarithmic result than what was *put in* would permit, we could easily do it. Thus:

log. of result = 1·3119321
log. of 20·508 = 1·3119233

Difference = 88

But we see from the tables that difference between the logarithms to 20509 and 20508 is 212, so that we must add to 20·580 $\frac{88}{212}$ths of ·0001, or ·00088 divided by 212, or ·00042, making our answer 20·50842.

But it may be said all this is very long and complicated; might one not as well multiply 21·9912 by 143303 and divide the product by 153664. It will be found, however, that with a little practice it takes but a few moments to take out a logarithm, or to find the number corresponding to one. A few points have to be carefully attended to, which we shall discuss with further illustrations of more difficult examples in our next.

Our Chess Column.

LEAVING to next week the further analysis of the "Two Knights' Defence," we give this week an illustrative game in that opening, and a singularly pretty end-game which occurred in actual play with "Mephisto."

GAME No. 2.

Played by "Mephisto" and Mr. Marriott, of Nottingham.

TWO KNIGHTS' DEFENCE.

White. MR. M.	Black. "MEPHISTO."
1. P. to K.4.	1. P. to K.4.
2. Kt. to K.B.3.	2. Kt. to Q.B.3.
3. B. to Q.B.4.	3. Kt. to K.B.3.
4. P. to Q.4. (a).	4. P. takes P.
5. Castles (b).	5. Kt. takes P. (c).
6. R. to K.sq.	6. P. to Q.4.
7. B. takes P.	7. Q. takes B.
8. Kt. to Q.B.3.	8. Q. to K.R.4.
9. Kt. takes Kt.	9. B. to K.3. (d).
10. Kt. to K.Kt.3. (e).	10. Q. to Q.4.
11. B. to K.B.4.	11. Castles.
12. R. to Q.B.sq.	12. B. to K.Kt.5.
13. Kt. to K.4.	13. P. to K.R.3.
14. P. to K.R.3.	14. B. to R.4.
15. P. to Q.B.4.	15. Q. to K.B.4. (f).
16. Kt. to K.Kt.3.	16. Q. takes B.
17. Kt. takes B.	17. Q. to K.B.4.
18. Kt. to K.Kt.3.	18. Q. to Q.2.
19. Q. to Q.R.4.	19. P. to K.Kt.3.
20. Kt. to K.4.	20. P. to K.B.4. (g).
21. Kt. to K.B.6.	21. Q. to K.Kt.2. (h).
22. R. to K.6. (i).	22. B. to Q.3. (j).
23. P. to Q.B.5.	23. Q. to K.B.2.
24. Q.R. to K.sq.	24. B. to K.4.
25. Q.R. takes B. (k).	25. Kt. takes R.
26. Q. takes R.P.	26. P. to B.3. (l).
27. Q. to R.sq.(ch.)	27. K. to B.2.
28. Q. to R.4.(ch.)	28. K. to Kt.sq.
29. R. takes Kt.	29. Q. takes Kt.
30. R. to K.2.	30. P. to Q.6.
31. R. to Q.2.	31. K.R. to K.sq.
32. P. to Q.Kt.4.	32. Q. to B.6.
33. Q. to Kt.6.	33. Q. to B.5. (m).
34. P. to Q.R.4.	34. Q. to Q.R.3.
35. Q. takes Q.	35. P. takes Q.
36. K. to B.sq.	36. R. to K.5.
37. R. to Q.Kt.2.	37. P. to K.Kt.4.
38. P. to K.Kt.4. (n).	38. P. to K.B.5.
39. Kt. to Q.2.	39. R. to K.7.
40. Kt. to B.4.	40. P. to Q.7. (o).
41. R. to Q.Kt.sq.	41. P. to K.B.6.
42. Kt. to Q.6.	42. R. to Q.2.
43. P. to Q.Kt.5.	43. Q.R. to K.2.
44. P. takes R.P.(ch.)	44. K. to R.sq.
White resigns.	

NOTES BY "MEPHISTO."

(a) Although, strictly considered, Kt. to K.Kt.5 is the stronger move, P. to Q.4 nevertheless gives the first player a good attacking game; it mostly leads to interesting variations of the Giuoco piano kind.

(b) If, instead of 5. Castles, White plays 5.P. to K.5, the game is transformed into a Giuoco piano. Black replies with 5.P. to Q.4, and by proper play he will withstand the subsequent attack of White, and retain the Pawn.

(c) If, instead of 5. Kt. takes P., Black plays 5. B. to Q.B.4, then we have the Giuoco piano variation known as Max Lange's attack, from the name of its author, in which White again proceeds with P. to K.5, as before.

(d) 9. B. to K.2 is given as best for Black, but we prefer the move in the text.

(e) Here 10. B. to K.Kt.5 is given by the books, with the object of preventing Black from Castling, if then P. to K.B.3, B. takes P. The move in the text, however, shows a steady determination to attack the Queen's side, as shown in his 11th and 12th move.

(f) 15. P. takes P. *en pass.* would have been correct; Black had nothing to fear from White's attack; *e.g.*, if, instead of Q. to K.B.4, 15. P. takes P. *en pass.* 16. $\frac{\text{Q. takes Q.}}{\text{R. takes Q.}}$ 17. $\frac{\text{Kt. takes P.}}{\text{B. takes Kt.}}$ 18. P. takes B. (if Kt. takes R. then B. takes Kt. with a far better game). 18. R. to K.B.4 19. $\frac{\text{R. to K.8 (ch.)}}{\text{K. to Q.2}}$ 20. $\frac{\text{R. to K.4.}}{\text{P. to K.Kt.4.}}$ with a good game.

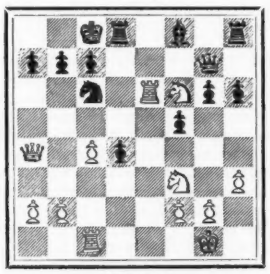
(g) This is weak. Black, by P. to K.Kt.3, tried to prevent the entry of the Knights into his game. The move in the text allows the Knight to play to K.B.6, and, if so inclined, afterwards to the commanding position on Q.5. Black ought to have played 20. B. to K.2, in order to prevent this, followed, perhaps, by P. to K.B.4.

(h) 21. Q. to K.B.2 was the correct move. White would then have obtained a very fine game by playing his Kt. to Q.5, followed by the advance of the Queen's Pawns.

(i) This is a very fine move, and initiates a combination belonging to the highest order of Chess play. We give a diagram of the position.

Position after White's 22nd move.

BLACK.



WHITE.

(j) The position is one of great difficulty for Black. If Black plays 22. $\frac{\text{Q. to K.B.2}}{}$ 23. $\frac{\text{R. takes Kt.}}{\text{P. takes R.}}$ 24. $\frac{\text{Q. to R.6. (ch.)}}{\text{K. to Kt.sq.}}$ 25. Kt. to K.5 winning.

(k) In reply to 25. R. takes Kt. instead of Q.R. takes B. Black would play 25. B. takes Kt.

(l) The only move to save the game. If, instead, Kt. takes Kt. (ch.), with the idea of winning another piece, then Black loses his Queen, *e.g.*, 26. $\frac{\text{Kt. takes Kt. (ch.)}}{}$ 27. $\frac{\text{P. takes Kt.}}{\text{P. to B.3}}$ 28. $\frac{\text{R. takes P. (ch.)}}{\text{P. takes R.}}$ 29. Q. takes Q.; whilst the move in the text wins.

(m) White still tries for a favourable chance by pushing his Queen's Pawns on to the Black King, which plan Black, however, frustrates by forcing the exchange of Queens.

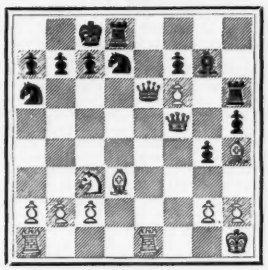
(n) Played to stop the advance of the Knight's Pawn; the Pawn cannot be captured, as, after P. takes P., and R. takes P., White plays Kt. to K.5.

(o) Threatening to Queen or win a piece if Kt. takes P.

Ending which occurred in an actual game played by "Mephisto" on Nov. 15, 1881, at 48A, Regent-street.

AMATEUR.

BLACK.



WHITE.

MEPHISTO.

Black to move.

White. MEPHISTO.	Black. AMATEUR.
. . . .	1. Q. takes Q.
2. P. takes B.	

This fine move, as will be seen, wins the game. If White had played, B. takes Q. instead, then B. takes P., would give Black the better game.

2. Q takes B.

Best! for if the Queen should retire on B.4. or R.4., then White would play B. takes R., and his Pawn on Kt.7. could not be prevented from Queening, which would leave White with a Rook ahead and a winning position.

3. B. takes R.

This again is right, as will be seen

3. Q. to R.2.

4. R. to K.8. (threatening mate).

4. Kt. to B.3.

If Black plays 4. P. to Kt.3. then White Queen's.

If White would play 5. B. to Kt.5, discovering check, and on the Black King moving to 5. Kt.2. 6. B. takes R. 6. Kt. to B.3. (best.) 7. R. to R.sq. 7. Q. to B.4. 8. Kt. to K.4. 8. Q. takes Kt. 9. B. to Kt.5. 9. Kt. to Kt.sq. (best.) Q. to R.; and White would have a very unsatisfactory game.

5. B. takes Kt. ch.	5. K. to Q.2.
6. Q.R. to Q.sq. (ch.)	6. K. to B.3. (If K. takes R. 7. R. to Q.8. mate.)
7. R. to K.R. 8.	7. Q. to Kt.3.
8. R. takes R. P. Queen's	8. Q. takes R. Q. takes B.

And White mated in two moves.

CHESS QUERIES.—[2]—A few errors occur on page 20 of KNOWLEDGE which may confuse young chess-players, viz.:—*Game No.* 1: White's 17th move should be Kt. to Q.2; Black's 34th move should be R. to Q.R.5. *In Notes to Game No.* 1: (h) White's 18th move should be P. to Q.6; variation A, Black's 19th move should be B. to R.2; (i) White should be if K. to Kt. 3; (n) This note I do not understand; *In Notes on "Chessikin"*: Black's 7th move should be Q. takes R.P.(ch.); Black's 8th move should be K.Kt. takes P.—WHITE PAWN.

[3]—I beg to call your attention to what I think is an error in Mephisto's note (s), KNOWLEDGE, No. 1, page 20. Having 16. B. takes P. / B. takes K.B.P., it says if White takes B. Black would win his Queen by P. takes P. should this not be R. takes Q.?—Yours, G. N. SHERBORN.—[Mr. Sherborn misunderstands Mephisto's note. It is at move 15 that White cannot take B. Of course, if White played 16. B. takes B., Rook would take Q.—CHESS EDITOR.]

BLACK PAWN.—Your game is very interesting, and shall appear, with some additional notes.

ILL-HUMOUR.—A great deal of what we commonly call ill-humour springs from the ill-conditioned state of the body. We familiarly talk about people "rising from bed on the wrong side," and there is a very suggestive meaning in the phrase. They may have been sleeping all night in a very badly-ventilated room, or have gone to bed after taking a heavy and indigestible supper. The consequence is, that in the morning, from want of pure oxygen, or from overloading the stomach, the whole organism is out of order, the nerves are on edge, and they rise fretful and impatient, and continue so throughout the day.—Paterson's "Health Studies."

Our Whist Column.

BY "FIVE OF CLUBS."

SO soon as we have accepted the general principle that in whist each player is to consider his partner's hand as well as his own, and that for this purpose each must inform his partner by every legitimate means of the nature of his own hand, we are at once able to decide on the proper way of conducting whist strategy. Were it otherwise, the first consideration of each player would naturally be the nature of his own hand. He would play so as either to make all his strong cards at once, or to adopt the course which seemed to him best for making them in the long run. If he had a short suit, he would try to get rid early of the cards of that suit, in order presently to trump the remaining good cards of the suit. And he would play his trumps solely with the object of making as many of them as he could. If every player followed such a course as this, the fortunes of the different hands would run very much as they do with good play, but the game would not be whist. It would be simply a chance game, each player's success depending on the number of good cards which happened to fall to his share, or on the fortuitous occurrence of short suits with opportunities for trumping them. The advantage of the scientific game is that it requires skilful strategy, and calls into action many useful faculties.

To tell my partner anything about the constitution of my hand, I must in the first place follow a systematic and generally understood method of selecting a suit to lead from, and, in the second place, I must open a suit so selected in the correct way.

Now, considering first the selection of a suit, we note that there is only one quality which, being common to all hands, can be adopted for systematic guidance. A player tells his partner nothing useful by playing out his good cards, even if he made the best use he could of them for himself by showing them at once. Leading from a short suit again is not only bad in itself—especially the atrocious lead from a single card which young players affect,—but it is not a method of leading systematically available, for not every hand possesses a suit of fewer than three cards. But every hand must possess a suit of four cards, at least—that is, a long suit. If, then, for no other reason, still for this, that, by so opening the hand, partner learns that one holds four, at least, of that suit (save in a few exceptional cases), the long suit would be a good one to lead, if that were always understood to be the meaning of the lead. But, apart from this, there is a manifest advantage—other things being equal—in leading from the long suit. This suit always has an element of strength, even though every card be small. Suppose, for instance, I have 2, 3, 4, 5 of a suit, an opponent have Ace, King, Queen, and the remaining six cards equally divided between the other players. Then, though we by no means advise a lead from 2, 3, 4, 5, if the holder of Ace, King, Queen drew three rounds, I should remain with 5; and, when trumps are drawn, that small card, if I get a lead, is as good as a trump; or, if I obtain a lead before all the trumps are drawn, that small card would either make a trick or draw a trump from the enemy as well as an Ace or a King. By leading from a long suit, and getting that suit so far exhausted that I have commanding strength in it, I secure an element of strength for my hand which comes next in efficiency to strength in trumps.

For the double reason, then, first and chiefly that in that way I can tell my partner the chief constituent of my hand; secondly, that by so playing I am likely to strengthen my hand, my first lead should be from my longest suit.

Of course, this rule, like all rules relating to a game so varied and complex as Whist, is not without exceptions.

I showed just now that a hand of four very small cards has a certain element of strength, which is wanting in a suit of the three highest cards; yet the latter has, of course, the greater strength. If you have two suits thus constituted, one long, but very weak, the other a three-card suit of great strength so far as the individual cards are concerned, you would be showing your partner best the chief constituent of your hand by leading from the shorter very strong suit, than by leading from the other. But a three-card suit must be very strong, or a four-card suit very weak, for the former to be preferred in this way. There are several reasons for this, besides the general reason that long-suit leads, followed systematically, instruct the partner best. A suit which is short with you is likely to be long with one or other of your opponents; and, if so, you are playing their game by leading it. Again, commanding cards of a short suit are more useful as cards of re-entry, that is to give you a lead later in the game, than they can possibly be if used early in the play of the hand. If your long suit is very weak, your partner will very soon find that to be the case, and by showing you where his strength lies, can serve your game, as a rule, better than you can serve his by opening a three-card suit, unless it be of absolutely commanding strength.

XUM

[1]—WHIST QUERY.—I have read your introductory article on Whist in this week's KNOWLEDGE, and was much interested by its good sense, but allow me to point out to you what seem to be two errors in the only illustration you give. You say, "I lead Ace, and follow with Queen of my best suit. My partner knows that I have the Knave and a small card left." This is by no means a necessary inference. It is a necessary inference, according to the best play, that the leader has not Ace, Queen, Knave, to five; and the play would be proper if he had Ace, Queen, Knave, to four; but he may equally have Ace, Queen, Knave alone, in which event he would also play Ace, followed by Queen.

You say also (and for this purpose I must assume the leader to have Ace, Queen, Knave, and a small one, and the partner to have King and two small ones, as in your illustration), "suppose that instead of following the recognised line of play for such cards, I lead the second round with my small card. My partner plays his King, and, let us suppose, wins the trick. So far as he can understand me at all he thinks I have three small cards of the suit left, and that the Queen lies with one of the adversaries." It is true that the lead would have simulated a lead from five, but why is it a necessary inference that the Queen is not with the leader. It is the commonest thing at Whist to lead from Ace, Queen, to five, Ace and then a small one. The only inference a partner from such play could draw (as the leader ought not to have both Queen and Knave, is that one or the other of these two cards was with one of the adversaries. Excuse my pointing out to you what seems to me to be worth your re-considering, if you ever collect your article for re-publication.—I am, yours, &c.

FRED. H. LEWIS.

[Our correspondent is, of course, quite right in his criticisms. What the partner of leader in the case supposed would *know*, if lead were made correctly, is only that *Knave* remains with his partner. If, subsequently, he finds that his partner has not been forced to lead from a short suit, he knows that a small card of the suit remains; but otherwise he cannot be certain of this. In the case where the play is incorrect, a small card being played after Ace, the inference would be that three cards remain, and that Queen and Knave are certainly not *both* among the three. It is two to one (nearly) that Queen lies with an adversary, but it is not certain.

"FIVE OF CLUBS."

Answers to Correspondents.

**** Communications which are to receive early attention should be addressed to the Editor of* KNOWLEDGE, *74 and 75, Great Queen-street, London, W.C.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

DIXON K. Thanks; your letter being marked private, we assume you do not wish your remarks in *Field* to be touched on here.—MARTIN HINTON. See reply below to "Old Christian," in so far as they relate to the general subject on which you have written.—M. B. ALDER. Later.—J. C. H. W. You ask whether the remark of Huxley's, quoted at p. 4, is not incompatible with his views as a materialist. Of one thing you may be well assured—it represents his real views. Prof. Huxley's views as a materialist are indicated in his admirable lecture on Descartes' "Discourse." He says there: "When the materialists stray beyond the borders of their path, and begin to talk about there being nothing else in the universe but Matter, and Force, and Necessary Laws, and all the rest of *their* grenadiers, I decline to follow them." Perhaps if you carefully ascertain what *are* Prof. Huxley's views as a materialist, you may find them to be less inconsistent than you suppose with his opinion on "True Science and True Religion."—JOHN HAMPDEN. We are quite "aware that you take a position antagonistic to all the professional astronomers and geographers of the present day." We are not, however, "disposed to earn the everlasting gratitude of the upholders of the Newtonian theory by showing the impropriety of" your "charges against it." You say "if the professors are too cowardly to defend their own system, they are not competent to write upon any subject." When the Newtonian system needs defence, it will be time enough to undertake the work. How could reasoning avail with the followers of Parallax? They believe that the earth is shaped like a flat wheel about 25,000 miles in diameter, and that the sun travels in a path always parallel to this flat disc, and some 4,000 miles above it. On a spring or autumn day the sun's path, they tell us, is appreciably a circle, vertically above the equatorial circle, from which circle no part of the flat earth is more than 6,250 miles away! Reasoning would be thrown away on any one who could even for a moment imagine this to be the case. Repeatedly they have been shown what should be perfectly obvious—that, were this the case, (i) the sun could never set; (ii) that he would vary greatly in apparent size as seen from different places, or from the same place at different hours, or on different days; (iii) that all the phenomena of the heavens would be entirely different from those we see; (iv) that there could be but one pole of the heavens, and that many other consequences would follow which are utterly unlike any actually observed. Their answer to this has always been simply vituperation and abuse of those who happen to possess some reasoning power. It would be utterly contrary to the dictates of common sense to find room for any arguments against the absurdities of Parallax and his followers, in any journal intended for sensible beings. We will not insult our readers so grossly. To yourself personally, as undoubtedly believing what you profess to believe (incredible though such credulity may seem), we would remark (though the rudeness of many of your letters would justify absolute silence on our part) that the phenomenon which you said we should never see—viz., the sun to the south of the east and west points—in Australasia, we saw repeatedly, and that on every day the sun moved in the southern skies precisely as he should move on the accepted theory, and as he never could move were the theory of Parallax true. But we must ask our readers' forgiveness for making so unnecessary a statement, even in this corner of the paper, and in the smallest type our printers use.—LOMBARDIAN. We know of no Woman's Rights Association. There is a Society for the Protection of Women and Children, Secretary, Rev. J. G. Roberts, 85, Strand.—J. J. G. Portions of your letter appear in the "Correspondence" columns.—B. H. T. (F. C. S.) Thanks for your encouraging note.—W. C. T. Have not yet seen the work you mention. Doubtless the communications you promise will be of interest to our readers.—G. G. I can well believe it.—H. AMBROSE SMITH. The article on the Fifteen Puzzle to which your refer appeared in the *Gentleman's Magazine*, but the date and number of the magazine we do not know. It must have been some time last spring, We were in Australia when the article was written. It forms one of a series of "Familiar Science Essays," which Messrs. Chatto & Windus are about to publish.—M. Y. W. One tail omitted in engraving. The editorial note at foot of left hand column of page 36 related to a letter which was at the last moment removed to make room for another relating to the November meteors. The note should have been removed too.—G. G. D. and L. T. F. Your replies to "Tyro's" query in letter 9, involve the same difficulty which has perplexed him in the text books and primers. These all speak of the axis remaining in the same direction (they all use the knitting-needle illustration also); but "Tyro" finds a difficulty, it seems, in understanding what is meant by "the same direction." The expression, "moving always parallel to itself," is equally perplexing to many beginners. Your answers are, of course, quite right.—A. HART. "To settle a dispute," we inform you that though the planets are often called stars, a star is not a planet. We do "get light from a planet;" but it is not the planet's own light; it comes from the sun, and is reflected by the planet.—A. H. SWINTON. The general idea that, after all, the sun's heat may be some form of electricity, without confirmation by facts or reasoning, would hardly justify us in publishing so long a letter as yours.—AN OLD CHRISTIAN. With some regret, we find ourselves obliged to decline publishing your letter on science and religion. So far as we are concerned, we should have been glad to do so, because it chiefly relates to our own remarks on the same subject. But you emphasise the differences which you believe to exist between "the notions of modern scientists, and the plain statements of the Bible," and we emphatically decline to discuss the question whether such differences even exist. We are only concerned here with the question whether scientific statements are correct or not. Any scientific opinion expressed in these pages you are free to oppugn on scientific grounds. When you find a line here suggesting the opposition of scientific opinions, sound or otherwise, to any religious opinion whatsoever, then—and not till then—you, or any man, may express or defend religious views in these columns. "In such a then, I write a never." To use the words of Professor Huxley, "Science and Philosophy, within the range of which lie all the topics on which we can venture to speak here, are neither Christian, nor un-Christian, but are extra-Christian." I entirely agree with you that "no intelligent Christian" (or Buddhist, or Mahometan, you might have added) "can raise an objection to modern science on the ground that it enlarges unduly our ideas of the vastness of God's domain in space, or of the immensity of the time periods during which He acts;" but the objections raised by many well-meaning persons to modern scientific teachings amount in effect to this. They do not see that to believe in the evolution of a solar system or of a system of such systems, no more amounts to setting the Almighty on one side in the name of universal evolution, than does belief in the growth of a plant, or even of the tiniest bud or leaflet. It may save correspondents, and it will save ourselves some trouble, to define our position precisely, which we may do as follows:—"With religion, properly so called, all the truths of science are in necessary accordance; and so far, what is taught in these pages will be essentially religious; but this journal is intended to spread scientific knowledge; and DOGMATIC RELIGION SHALL HERE BE NEITHER ATTACKED NOR DEFENDED."—BIRD'S EYE VIEW. You may be very right, but this is hardly the place for advocating your views. *Remaining letters unavoidably crowded out.*

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